THE LANCET.

VOL. II.] LONDON, SATURDAY, JULY 28. [1832-33.

CLINICAL LECTURE

BY

JOHN ELLIOTSON, M.D., F.R.S.

PROFESSOR OF THE PRINCIPLES AND PRACTICE OF MEDICINE

IN THE UNIVERSITY OF LONDON,

Delivered April 2, 1832.

Most of the cases, Gentlemen, that were presented or have died since the last lecture, afford considerable matter of interest, but I must pass over some that are curious on account of one of great interest.

PORRIGO LUPINOSA.

The first case to which I will allude was that of Elizabeth Roach, who was admitted for porrigo lupinosa—that kind of porrigo affecting the head, in which there are numerous granules, so that the appearance is that of lupin seeds, hard granules, dry, and, I believe, containing earth. I understand that a considerable portion of lime has been found in them. In this form of the disease there is generally no inflammation, and stimulants are therefore applicable, and are found very useful. Nothing can be worse than to apply stimulants indiscriminately to affections of the skin, for the skin is occasionally, in the same disease, in a very different condition. Sometimes there is no inflammation; sometimes, if there be no inflammation, it is excessively irritable; and sometimes it is in a languid condition, so that stimulants are absolutely required.

Treatment.—In this case, which occurred in a girl aet. 16, and had existed twelve months, I ordered the head to be shaved, and what is often very useful in this disease, two grains of calomel, which she took night and morning, until her mouth became tender. The stimulant I applied was ung. picis, and in the particular state of her head, there being an entire freedom from irritation, it was productive of very great benefit for a certain time. All these crusts came off, and the head looked comparatively smooth, but the application at last, having done all the good it could, was beginning to do harm. The head became hot, and an inflammatory state came on. If I had continued tar ointment under these circumstances, I should have made bad worse, there being a copious discharge of pus, and the whole of the head being red and heated. I therefore omitted it altogether, and ordered simple cold water to be applied. The mouth had become sore from the calomel, but that presented no objection to the cold water, provided it was applied only where the parts were excessively hot. This method speedily brought down the inflammatory state, and the patient was so much better that she went out. The case was not cured, and it will require constant attention for many months, and after all perhaps the disease will not be entirely eradicated. It subsided however by stimulating treatment in the first instance, and sedative treatment, if I may so call it, in the second; and it is very possible that stimulating treatment will, after a time, be again necessary.

ECZEMA.

Another case was presented in the same ward, of disease of the head, which would be called *porrigo* by Willan and Bateman; but I think there is no doubt that many diseases of the scalp, called by Willan and Bateman porrigo, are really other diseases. I hardly ventured to say so till I heard that a French physician, Biette, was convinced of the same thing. Many cutaneous diseases affect the head as well as porrigo, which have no contagion in them, and many of them have improperly been called porrigo by Willan and Bateman. This appeared to be a case of eczema of the head.

There was a considerable discharge of thin watery fluid, which concreted into fine minute crusts. I frequently found some on the head, exactly of the appearance of eczema in other parts of the body.

Treatment.—It was necessary here to conduct the treatment on antiphlogistic prin-

ciples. She had had the disease four years. Leeches were applied every other day behind the ears, and cold water, in addition to which she took five grains of hydrarg. c. creta twice a day. Accidental inflammation took place—a degree of inflammatory fever, and there were signs of inflammation in the head, and inflammation of the stomach and intestines. There was inflammation of the mucous membrane of the intestines, inducing diarrhœa on the one hand, and of the head inducing delirium on the other. This was subdued in the common way by leeches and blisters applied to various parts, and then the head was attended to again, and similar treatment adopted as at first; but at last she became free from all excitement, the scalp fell into a languid state, and the tar ointment was applied with great benefit, so that I was able to present her likewise on Thursday last.

You will find it of the greatest importance to vary the treatment in these local chronic diseases of the skin, as well as in those much more diffused throughout the body, accordingly as there is inflammation or great irritation, or neither—for the applications which would be right in one instance would be wrong in another.

JAUNDICE.

There was a man presented who came in with jaundice, and the case was instructive, as showing that jaundice may be a mere inflammatory disease,—nothing more than an accidental symptom of hepatitis. This man's chief apparent complaint was the yellowness of his skin, the yellowness of the conjunctiva, and the yellowness of the roots of the nails. He said he had been ill three months, but particularly ill only during the last six weeks. Of course the motions were rather white, and the urine the colour of porter, showing a yellow tinge if you bent the vessel down a little so as to see through a thin portion of fluid. But on examining the right hypochondrium, I found considerable tenderness, proving that the case was clearly one of hepatitis, inflammation of the liver, and I regarded it simply as such.

Treatment.—He was treated by cupping the epigastrium to twelve ounces, by giving him ten grains of blue pill twice a day, and putting him on low diet. By this means the tenderness all went from his side, and with it went away the jaundice. You will find continually that jaundice is nothing more than a symptom of hepatitis. You may have hepatitis without jaundice, and I presume it is from the accidental circumstance of the inflammation affecting the parts where the ducts are, that you sometimes have jaundice in hepatitis. But it is worth knowing that jaundice is often an acute or sub-acute affection, and yields to the common treatment of inflammation.

GASTRODYNIA.

There was a girl presented who came in on account of gastrodynia, and as it is a common affection it may be worth while to say a few words on the case. She was twenty-three years of age, and had been ill she said a twelvemonth.

The symptoms were—pain at the pit of the stomach over the epigastrium, coming frequently with great violence, and darting through to the back, and occasionally she discharged a watery fluid into the mouth. It was a case of *gastrodynia* and *pyrosis*; sometimes there was a discharge of fluid, and sometimes none.

Treatment.—One of the best medicines for this affection, when there is no inflammation, is hydrocyanic acid. This sort of case is sometimes very obstinate; it is very common in young females, and sometimes is so obstinate, so violent, that it has been supposed indicative of organic disease, but afterwards they have recovered perfectly. Of course hydrocyanic acid is not a specific for it; there are cases in which it will fail, as other things fail in every disease. She began with two minims three times a day, and it was increased to what is a very considerable dose, and one which you will not often be able to reach. It was gradually increased, from the 6th of February to the 19th, up to eight minims and a half for a dose. This must be considered a very large dose, but it was given with perfect safety, because after we had arrived at seven minims it was increased only half a minim at a time. However, this medicine did not do her good till we had arrived at this large dose, and when we arrived at it, from the long continuance of the pain, there was more or less inflammatory state of the stomach, and it was in vain to think of subduing that by hydrocyanic acid or purgatives. It is necessary under such circumstances to take away blood from the arm or the region of the stomach, and in the latter case leeches are the most eligible mode, the parts being too tender to bear cupping-glasses. Leeches were applied to this patient several times, and we continued the hydrocyanic acid. She had no tenderness when she came in, and when it came on at last there was no inconvenience from the medicine. While however I mention this dose, it is right I should tell you that I have known a patient who would not bear at first more than a fourteenth of a minim for a dose, but who was cured of a complaint with doses not much increased beyond that.

ABSCESS OF THE LABIUM.

A woman came in the course of the week who was represented as very bad,—as in a dangerous state; but on examining her it turned out that there was nothing but an abscess of one of the labia. She said she was a new married young woman, and her complaint had suddenly begun at six o'clock in the morning. There was a quantity of matter, and she said that her bowels were confined. We could not feel fluctuation satisfactorily, but it was evident there was an abscess of one labium; and on being poulticed, it broke, and the case terminated.

HYPERTROPHY OF THE LEFT VENTRICLE AND AURICLE.

There was likewise a woman presented with hypertrophy of the left ventricle. The symptoms were, great irregularity of the pulse, both in force and frequency, difficulty in lying on the left side, palpitation, and when the heart gave the strongest strokes, I may mention, that the stroke was dreadfully violent, that the heart thumped against the side, and raised the stethoscope. There could be no doubt of the left ventricle being hypertrophied. These were her symptoms, together with difficulty of breathing and œdema of the legs. There appeared to be no disease of the openings of the heart, for there was no preternatural sound.

Treatment.—She received the greatest possible relief, though a cure is out of the question, by a combination of ten minims of tinct. digitalis; the same quantity of tincture of squills, one drachm of nitric ether, and half a drachm of acetas potassæ, every eight hours. I have mentioned several times in the course of the winter, that a medicine of this description often effects the greatest relief, although it will not succeed in curing the complaint.

SCIRRHUS UTERI—MALIGNANT CHOLERA.

I now proceed to the case of the woman from whom the parts now before me were taken.

A woman, ætat. 44, married, but who had never had any children, was admitted on the 22d of March, whose catamenia had ceased for five years. She complained of pain in the loins and lower extremities and hips, and general feverishness. She had also discharge from the vagina of a yellow colour and a very offensive odour. This had continued six months. I examined her carefully by the vagina, and found the os uteri sound, nor could I discover disease of the neck of the womb. From the pain in the loins and hips, and this discharge, there was every fear of uterine disease, but I did not feel justified in saying there was disease of the womb; for both the os uteri and neck were sound, so far as I could judge. I would not say there was no disease of the womb, but only stated there was none to be felt. You have no right in any case to say, that a person has not organic disease, for it is astonishing what organic disease may be found after death, when there was no ground for asserting it before death. You may believe it, but you cannot assert it. One has seen a large portion of the brain softened from an abscess without any disturbance of the mental faculties before death, and without paralysis, and such a person has died suddenly. We can hardly say that any of us here are free from organic disease, though I hope we are; yet organic disease may exist to a certain extent, frequently without producing symptoms. This woman had great structural disease of the womb, but unless you can feel disease of the os uteri, or can feel the womb enlarged, either by the vagina or the rectum, you cannot say that such is the case. This woman had likewise had a cough for six months, and was evidently spitting pus; she was likewise deranged, wrong in the head, was frequently incoherent, evidently labouring under a degree of mental alienation.

Now it so happened, that on Friday last, when I was going round, I observed the woman's countenance to be altered. There was the slightest possible suffusion about the eyes, and an expression of the face which I cannot describe. There was a weakness, a tremulousness, and a very slight hoarseness in the voice, which appeared to me exactly like the cholera voice. On feeling her pulse, I found her hands cold, and the pulse exceedingly feeble. These things struck me very much, and I inquired if there had been any vomiting or purging. She declared she had been purged all night, every ten minutes, but the nurse declared there had been no purging at all, and I could not learn whether there had been vomiting or purging. The symptoms were so suspicious, that though I would not pronounce it cholera, yet I believed it was. Just as in all probability there was disease of the womb, though I could not prove it, nor could I think of asserting it, so I would not assert there was cholera, but I had great fear about it, and considered it a matter of prudence to remove her from the ward. She was removed, and died, I understand, at eight o'clock the following morning. It appeared from all I could learn that she had been a dram-drinker; she had the appearance of it when she came in, and her breath had that particular smell which a great number of dram-drinkers have. She said she was very weak at her admission,

and half starved; and sometime afterwards she said she had had nothing to eat since she came to the hospital; however, she was allowed a pint of porter a day, and meat diet. The account I have given you from memory, accords exactly with that which appears, I see, in the entry book.

About two hours after her admission into the cholera hospital, she had a fluid stool containing some bile. She did not vomit, nor was she affected with cramps, neither was there blueness of skin. At seven o'clock great difficulty of breathing came on, and she gradually sank and expired at eight in the morning.

Post-mortem Appearances.—On opening the head the dura mater appeared more opaque and thick than usual, and the outer surface of it was in a state of congestion. The arachnoid was very opaque, and had on its surface a flaky deposit. This is the dura mater very much thickened, and there is great congestion of blood on the inner surface. This is not at all to be wondered at, as she was wrong in her head. You very often after insanity discover no disease in the brain itself, but disease of the membranes; not that the disease of the membranes has been the immediate cause of the insanity, for they are not the organ of mind, but the disease in the brain has extended to the membrane and produced visible marks of disease there, though they have not appeared in the brain. The disease of the membranes had disturbed the function of the brain by vicinity. The brain was unusually firm, and litmus paper placed on the surface of a section was reddened. At the base there was considerable effusion, probably one ounce and a half. I mentioned that she was spitting pus when she came in, but she would not allow me to use the stethoscope carefully. She was a restless woman, and being a little cracked, she would not allow me to keep the stethoscope steady upon her for a moment. However, the substance of the lungs was almost completely disorganised, and filled with tubercles, and containing many cavities. The heart was healthy.

The uterus was greatly diseased—affected with cancer. These parts of the womb are in a state of cancerous ulceration. Here is scirrhus and open cancer by its side, very offensive, as you will perceive. You see the fundus of the uterus irregular, having a scirrhous deposit. The walls of the uterus are much thicker than they should be at the fundus; they are a little thicker lower down, but at the fundus you see the great thickness. The fundus, too, is ulcerated; that part of the walls of the uterus which forms the upper part of the cavity of the womb; and what is very rare indeed, here is the os uteri and the cervix, as I mentioned, not at all contracted or thickened. This must be considered a rare specimen of disease of the womb, for, in most cases, disease of this organ, whether it be cancer or malignant ulceration, begins at the neck, and that part, or os uteri, ulcerates, while the fundus may be sound, and, as a general rule, the ulceration proceeds more rapidly at the neck and the os uteri than anywhere else, that part being first affected. I could pass my finger around the os uteri, it was so perfectly smooth, but the discharge coming through it was horrid enough. Here is a fresh cut, here is scirrhus softening, and here it is as hard as cartilage.

The kidney was perfectly healthy, and so were many other parts. I was not present at the examination, but with respect to the abdomen, the stomach contained much mucus, as well as the duodenum, in which also there was some bile. The intestines, from their commencement at the duodenum to their termination, throughout their whole tract, were distended with fluid of a milky-whitish colour, containing white flakes, and having much the appearance of gruel. Now this is exactly the kind of fluid which is found in the stomach and intestines of many persons, in fact, the greater number who die of cholera. There were no fæces, I understand, in the intestines—a lumbricus was found, but no fæces. The glandulæ solitariæ were enlarged throughout the whole length of the intestines, and the fluid contained in the stomach and intestines reddened litmus paper.

Now as to these circumstances, twice last week I was present at the inspection of persons who died of cholera, and the alimentary canal was filled with exactly such fluid as this, and litmus paper, whether it was dipped in the fluid of the stomach or the intestines, was reddened in both cases; but in one case I distinctly noticed that the litmus paper was reddened more at the stomach and upper part of the intestines, than anywhere else. As we went down, the paper was less and less reddened, till at last it was not reddened at all, and on applying turmeric paper where the litmus was not reddened, that was not discoloured, so that the fluid was not alkalescent. I mention this, because it has been observed that the fluid in cases of cholera is alkalescent. It is certain from the case of cholera before us, and the two cases to which I have alluded, and which undoubtedly were cases of cholera, that the contents at the upper part of the intestines were acid, and proceeding to the lower part in one case they were neutral. I recollect, that in trying the stool of another patient who was under the care of Dr. Roots, and who had cholera in the hospital a fortnight

ago, the litmus paper was reddened by the stool.

I have no doubt whatever, now that the body has been opened, that the case before us was one of cholera. I had a strong suspicion of it during life, but there was no discharge by which I could ascertain it, but the intestines were filled after death with a white fluid containing flakes, having all the appearance of gruel.

In the case that I saw examined last week, the surface of the stomach and intestines was of a fine pink colour, a bright rose colour, and I should say, clearly showing an inflammatory state. The vena cava inferior and superior was immensely distended, in such a state of distention as I never saw before: whether it was so in this case I do not know. The blood in one case, I forget how it was in other, was grumous, half coagulated in black lumps. The blood in the left side of the heart had coagulated just as usual, the fibrin had separated from the red particles in some parts, so that you had just the white collection of fibrin that you have every day. The remarkable circumstances were, the distention of the left side of the heart and the venæ cavæ, and the blood being in this half-coagulated state, exceedingly grumous.

Remarks.—With respect to this disease, I have really seen enough to convince me, *that it is an affection perfectly new in this country, and that it is the same disease that has prevailed in India.* On these points I am perfectly satisfied. I have been physician to this hospital fourteen years, and I never saw the disease before the last month, never saw anything at all like it. It is quite in vain to say that it is a species of common cholera, because in the slightest forms of this present disease you have, generally, this peculiar white matter. Now in the disease to which the name cholera is given in this country, you have a discharge of bile; the motions are bilious—it is principally a discharge of bile; but in this disease you have no bile discharged, except at the beginning or close of the disease. When the vomiting and purging first occur, the bile already in the duodenum and intestines is discharged, but when the disease is fully formed, no more bile in general goes into the intestines; and the slightest cases of this description are characterised, not by bile, but by these motions. Cases so slight, that they do not cause death, or cause the patient to be ill above a day or two, are attended by these particular motions. If this were only a modification of cholera, slight cases ought to be like ordinary cholera, and we should have bilious stools, but that is not the case. A gentleman present at a dissection, who has been in the way of catching the disease, has merely purging, but the diarrhœa is characterised by white fæces. As to the name cholera, nothing can be more absurd, because it means bile flow, and in this disease there is anything but bile-flow. *Leucorrhœa* would almost be a more appropriate name if it were not already engaged.

You know that the symptoms observed in this disease prevailing now in this country, are diarrhœa and vomiting; sometimes diarrhœa existing for a larger or smaller number of days before any other symptom comes on,—diarrhœa of this white description, watery, not feculent, and containing these white flakes. Sometimes this disease begins all at once, without any previous diarrhœa, and when it does begin, there are generally violent spasms of the calves of the legs and thighs and trunk, and sometimes in various other parts. The nails and ends of the fingers frequently become blue, and sometimes before the body is dead, it is blue from head to foot. Sometimes the fingers also become shrivelled, and the eye sinks into the head, so that there is a dark circle round it. The voice becomes feeble; great prostration of strength is felt; the extremities are cold; the pulse is not to be felt perhaps at all at the extremities, although it may at the wrist, and patients may still experience vomiting and purging. When patients do not die immediately, the symptoms become fainter after a time, and signs of congestion take place in the head, but I have not yet seen that stage: the patient looks as if he were labouring under typhus fever. However cold the body may be, many hours after death it often becomes exceedingly warm. These are the things usually observed. Even the fingers and toes often move after death. You will not find perhaps two cases of cholera alike; some patients are not blue, and there are great variations, but these are the general symptoms, and I have seen most of them myself in this epidemic.

[The learned Professor then read several extracts from a pamphlet entitled *Cholerica Indica*, published by Dr. W. H. Foot, of Edinburgh in 1825, for the purpose of showing the perfect identity between the present epidemic and that which prevailed in India.]

Treatment.—With respect to the treatment, we had better say nothing about it. It is my own private opinion that no good has, on the whole, been done; that if all the patients who have been treated had been simply put into bed, and made as comfortable as possible, we should have had as many recoveries as with all the means that have hitherto been used. I hope that those who attend to cholera patients, will go on making a cautious trial of some new means, or employ the means already in use

in some fresh mode; for I cannot but think that the mortality has not been diminished by any measures hitherto adopted. I feel satisfied that many have been over-treated—have had measures used quite indiscriminately and in excess. I may add, that *I have seen enough to convince me also, that the disease is contagious,*—may be produced by emanation from a system affected by the disease; though I do not doubt that it may be also taken without contagion,—that something in the air will produce it, without that something having proceeded from a diseased body. It is a remarkable fact, that Dr. Prout, who has long been weighing the atmosphere twice a day, found it suddenly become very heavy exactly at the time that cholera broke out, as if something were added to the air, and that it long continued so heavy, up to the present time.

I may also mention, that for the last three months, Dr. Prout has observed an extraordinary change in our secretions. He has found the perspiration, which is mostly acid, to be exceedingly so: the saliva, which, except in some cases of dyspepsia, and before death from organic disease is neutral, to be acid; lithic acid and lithate deposits, to disappear, or be greatly diminished in the urine, and an acid more like oxalic to prevail in it, and the morbid deposits of patients, which before were usually compounds of lithic acid, now to be the oxalites. Something evidently exists in the atmosphere at present, which exerts a strange power over, at least, the human body.

I may add, that in the woman whose case I have mentioned, the bladder was found contracted, and contained a thin fluid, with white flakes, like that which was discovered in the alimentary canal.

*** Our reporter has requested us to append the following note to this part of the present lecture:—Since the cholera has again grown violent in London, Dr. Elliotson has employed the *saline treatment*, and has a high opinion of it. The cases he has lost were, we understand, of extreme rapidity, and were such as he says that he should henceforth feel justified in treating with far larger and more quickly-repeated doses of the alkaline salts, than those given in ordinary cases.

The very interesting fact may be here added, that when cholera appeared again in St. Thomas's, three or four weeks ago, after its entire absence, during the intervals it broke out in a patient lying ill of some other disease *in the very bed in which the patient whose case is detailed above, lay*, though the bed had been scoured and cleansed in every way. The cholera has now again entirely ceased in the hospital, and, indeed, is hardly, if at all, seen in the Borough. It may here be stated, and the circumstance is a curious and important one, that Dr. Prout discovered the sudden increase of weight in the atmosphere alluded to in Dr. Elliotson's lecture, to occur almost *on the very day* on which cholera broke out in London.

LONDON HOSPITAL.

CLINICAL LECTURE

BY

DR. BILLING,

Delivered Jan. 28, 1832.

HYSTERIC DISEASES.

I HAVE a few cases of hysteric disease to remark upon to-day, and in the first place that of S. S., who has been mentioned already (see LANCET, last volume, page 583), which you may take as the prototype and beau ideal of hysteria, and which is worthy your continued attention, as you will in your practice meet with cases of the kind not unfrequently, though, fortunately for the patients, few so bad—bad as to the sufferings of the patient. She has been in the house since Nov. 18th, and has already had many of the remedies which have been found efficacious in similar cases, with no good effect, at least not permanently; but I shall persevere in the treatment, both as a duty to the patient, and because I wish you to have a thorough acquaintance with the phases of such a malady. We cannot treat her as an out-patient, as her home is in the country, but many of these patients are much better out of the hospital, as having more exercise and air. You have seen that without chiding her and the nurse, we cannot keep her out of bed, though she is the picture of health; most patients in her state have a propensity for lying on the back, and if they remain in bed, the heat produces relaxation and increased irritability of the nerves of the spine and sacrum; they ought to be kept out of bed, and every effort made to break them of bad habits they may have acquired; their fingers should be employed with some kind of light work, or a book, &c. as much as possible; and when it is necessary (which sometimes is the case, on account of bearing-down pains) that they should keep a good deal in a horizontal position, they ought to lie on a firm sofa, or on a thin mattress, or on a board.

When I last mentioned the case of S. S., which was on the 18th December, she was taking *ferri carbonas*, which had the good effect of removing the nervous tremors and headache, but did not prevent the recurrence of the fit, which occurred at its usual period; you had a specimen on the 13th, as she was attacked, just as we went into the ward, with a fit of hysterical epilepsy, violent convulsions, and insensibility, knocking herself about, and trying to bite, the eyes squinting obliquely upwards to the utmost; cold water was applied to the face and epigastrium, smelling salts to the nose,

&c., *secundum artem*; but these have little effect on the duration of the fits; your efforts must be directed to preventing their recurrence. The principal thing during the fit is, to prevent their injuring themselves; and this is difficult, for even the necessary force used in holding them down, makes them feel the next day as if they had been beaten all over. You know that a person who is sensible, will cease to exert the muscles when the resistance gives pain, but in convulsive fits, the patient, not being conscious, exerts the muscles to their utmost power, appearing to have extraordinary strength, and thereby strains them, so that the day following they are all in pain.

After this she had a better excuse than ever for sticking to bed. You had then an opportunity of seeing how little real benefit is derived from bleeding, even in such a plethoric subject as this; and I must repeat to you my reasons for resorting to it, contrary to my usual practice in hysteria. The effect of the pains of the limbs, almost like acute rheumatism, and her other usual sources of irritation, produced heat of skin, increase of pulse, and headache, with permanent flush, and an occasional strabismus, besides which, the usual periodical return of the catamenia had been retarded; under these circumstances it would have been carrying too far the objections to the secondary consequences of bleeding, to have run the risk of the brain being affected; and I must warn you, that the effect of the cerebellum becoming inflamed in such a case would be, the occurrence of nymphomania, the most melancholy visitation with which a modest woman could be afflicted, a disease in which the sensations are all painful, and which no persons who know the nature of it would speak or think lightly of, any more than of hydrophobia. I ordered her *to be bled* from the arm ℥xvj, and to have *a blister* on the nape of the neck, which relieved her head; but having still much pain in the pelvis and lower extremities, I had her *cupped over the sacrum*, at the origin of the nerves of those parts, and afterwards a *blister* applied; her pains were relieved, and the catamenia were restored. You ought to know that amenorrhœa is frequently relieved in plethoric habits by bleeding from the arm, or cupping the sacrum, or leeching the pubes or thighs,—as it is, in delicate habits, by stimulants, aromatics, and opiates; thus I have constantly to remind you and to exemplify, that diseases with the same name require opposite remedies.

As the fits were threatening on the 20th, and she once had been benefited by the *ol. terebinth.*, I ordered it in *enema*, to avoid irritating the kidneys, and she appeared better, but had a fit on the 27th. In the beginning of this month, she became more hysterical than ever, lay in bed constantly, had two or three fits a day, and had complete hysterical loss of voice; she also got that hysterical tenderness of abdomen which simulates peritonitis, but is entirely neuralgic, not inflammatory, and hence so often relievable by carbonate of iron. I took this opportunity of pointing out the diagnosis to those gentlemen who were with me in the ward on the 13th instant. In the first place the abdomen was exquisitely tender; indeed more so than in peritonitis, for in peritonitis the skin is not tender as in hysteria; but there was no confinement of bowels; the tongue was clean and the skin soft; the pulse 132, which, I remarked, was too quick for inflammation; the pulse of an adult is scarcely raised above 120 by inflammation, but these nervous patients have it from 130 to 160, and when I feel this nervous flutter, it relieves my mind from apprehension of danger, with the other absence of symptoms which I have just stated. After this explanation, I pointed out that she was sobbing, and just on the point of going off into a fit, as she had done several times lately whilst we were in the ward, and that I would try if the effect of making her exert herself would not be to keep it off; I told her rather suddenly, that I insisted on her keeping out of bed in the day-time, and that if she could not stand she must lie in her clothes on the bed, but not in it, and that she must exert herself, as it was the only way to get well, and so persuaded her out of the fit for that time; I at the same time desired the gentlemen, to convince them there was no inflammatory disease requiring active treatment, that she should take only *aquæ menthæ* ℥iss *ter die*, and to relieve the uneasy sensations in the abdomen, she had a mustard poultice applied in the evening: she has been going on very well with the aqua menthæ ever since. On the 17th I ordered the watering-pot douche to the loins, which strengthens the nerves, and she would now be better, but that it is just the period at which the fits usually recur, and she has had two or three lately; you shall see me persevere, however, and though I do not expect to be able to cure her whilst she remains in the hospital, as it is not justifiable to occupy a bed so long with a chronic case, she is by no means incurable, and may get well more rapidly at home than you would suspect.* You must not give way to the idea that the disease is mental, though mental exertion, you see, influences it, and you must turn that to

* This patient was dismissed in the middle of March, still subject to fits, after having had fœtid, antispasmodic, mercurial, and other remedies; she appeared in perfect health, as when admitted; if any thing rather stouter.—REP.

account in treating these cases; the toothache is not mental, yet if very busy, a person forgets it for the time. Matrimony produces a total change in the thoughts, produces fresh cares and occupations, and hence has a powerful moral effect in this disease, independant of utero-gestation or other considerations.

AMENORRHEA.

I have now some cases of amenorrhea to allude to; first, those which are simple; secondly, those connected with vicarious hemorrhages.

S. G., ætat. 19. Dec. 8. Complains of pain and giddiness in the head; faintness and debility; pain in the left hypochondrium; uneasy sensation in the throat (globus); feet cold; pulse, tongue, and skin, natural; appetite good; bowels confined four days, and are generally costive; has been ill three weeks, but catamenia absent three months.

Pil. hydrarg. Pil. aloes c. myrrha aa. gr. v. *alt. noct.*
Magnesiæ sulph. ʒi;
Liq. antim. tart. ʒss, *ex*
Aquæ ʒiss *ter die.*

This was a case of simple amenorrhea brought on by the most common cause,—cold or wet feet during the catamenia, and eventually producing the train of hysteric symptoms, with debility, preventing her going through her work as servant; she was not one of those who require to have the occiput shaved. Besides having globus hystericus, she was just beginning to get that white appearance about the lips and mouth which would have terminated in chlorosis. You find that in these cases, sympathy with the uterus brings on a sluggishness in the intestines, which causes costiveness, and the most effectual emmenagogues are those tonic laxatives which renew the activity of the canal,—the blue pill by increasing the biliary secretion, and the old-established Rufus pill, in which the myrrh and aloes are both tonic, the aloes tending to keep up that action in the lower intestines which the blue pill does in the upper region. Sometimes blue pill, or calomel, is sufficient for all, and thus the sympathetic reaction on the uterus becomes emmenagogue. On the following day, finding that though the bowels had not acted there was no inconvenience, and that the abdomen was neither distended, nor tender on pressure, I thought it better to give her only the emmenagogue laxative, and at the same time *ferri carbonatis* ʒj *ter die*, instead of the mixture, as it would much more improve the chlorotic state; and, besides, you will often find the iron a most powerful means of keeping the bowels regular, when their sluggishness is from want of tone; another thing which decided me in not forcing the bowels with purgatives was, that her head was much better, which showed she was not suffering from not having had a motion for five days; and you will often find a patient relieved by the influence of a medicine on the primæ viæ, sometime before it produces a motion; she continued these medicines for a fortnight, and then left the hospital quite well, and able to resume her employment.

M. D., ætat. 20, unmarried. Dec. 2. Complains of palpitation and dyspnœa on walking or going up-stairs; pain in the left leg; debility; she has nausea, and flatulence after eating; tongue furred and pale; bowels confined; amenorrhea; is very pallid, and says she has suffered privations.

Mist. ferri comp. ʒiss *ter die.*

This was another case of simple chlorotic amenorrhea, in which the tonic myrrh and iron mixture, with good diet, kept the bowels regular, and restored the health and strength in three weeks, without any other prescription.

VICARIOUS MENSTRUATION.

There are two cases of vicarious menstruation. It is not a very uncommon thing, when there is amenorrhea, for the patient at the regular periods to bring up blood, either from the stomach or lungs, or have epistaxis; sometimes this hemorrhage takes place from an organ not previously diseased, but suffering from sympathy with the uterus, as in the young woman J. C., who went out sometime ago well, after having had spitting of blood, vicarious, with amenorrhea; she first had hysteric cough, and then hemorrhage from the irritated bronchia; the treatment in these cases being, emmenagogue medicines, combined with dilute sulphuric acid, and other cooling astringents, and quiet when the hemorrhage occurs; but in the case of E. C., who is now in the house, there is more difficulty, as the hemorrhage takes place from a lung previously diseased.

E. C., ætat. 32, unmarried. Dec. 30. Amenorrhea for eight years. She has had cough for two years (from exposure to cold after being called out of bed in the night), during which, at the usual periods of the catamenia, she has either epistaxis, or spits blood; leucorrhea profuse; expectoration now not scanty, but tough muco-pituitous; rhonchus sonorus gravis and sibilans; crepitation and dull respiratory murmur, with bronchial respiration, and dulness on percussion inferiorly on right side; legs

œdematous; tongue clean; pulse 74, soft, and regular; bowels confined; urine scanty.

Decoct. senegæ ℥x;
Syrupi papaveris albi ℥ij;
Zinci sulph. gr. ss *ter die.*
Pil. hydrarg. Pil. aloes cum myrrha; *aa.* gr. v, *alternis noctibus.*

The explanation of the symptoms is this: This woman has had leucorrhœa and amenorrhœa for years; she caught cold two years ago, and had, in consequence, peripneumony of the right side low down, in which place hepatisation of the lung exists, but in a very passive chronic state, as her pulse, &c., indicate; she coughed and expectorated from chronic catarrh, for which I prescribed the tonic anodyne and expectorant mixture, and, for the uterine symptoms, the pills; there was a slight crepitation on the edge of the hepatised portion of the lung, but as there was no acute symptom, it did not call for any active depletion. The medicine at first produced nausea, and relieved the cough; her mouth became sore in less than a week, and, as it did so, her pulse became hard. On the 13th, the usual period, she brought up about a teaspoonful of blood; considering the equivocal state of the lung, I had her bled to ℥x, which relieved her much, and she brought up no more blood, only the expectoration was discoloured, of a brownish red. You must observe that much of her dyspnœa, palpitation, and pains, is nervous and hysterical, as well as the globus and low spirits, depending on the old uterine affection, which make me less anxious about her, though the state of the lung must require constant care.

I have ordered her now ℥ij *cubebs ter die*, which, besides being indicated for the leucorrhœa, is as good a medicine as she can take for the chronic catarrh.*

Some years ago I was consulted by a young lady who had had, at first, vicarious hæmatemesis every month, but the hæmatemesis had become more frequent, accompanied with pain at the pit of the stomach, dyspepsia, constipation, and dropsical symptoms. I prescribed *leeches to the epigastrium, magnesiæ sulph.* ℥j, *with dilute sulph. acid.* ♏xv, *ex. inf. rosæ comp. ter die*, which stopped the hemorrhage, kept the bowels in order, and removed the pain; after this I gave small doses of *ol. terebinth. ter die*, which improved the state of the mucous membrane; and, subsequently, the *emmenagogue pills*, with *carbonate of iron*, restored the catamenia and the health of the patient.

* This woman left the hospital in the middle of March, having been free from cough for some time, but the hysterical symptoms and leucorrhœa continued, for which she was directed to attend as an out patient; the *cubebs* had been changed for *mistura ferri comp.* on the 24 of February.—*Rep.*

CASES OF PETECHIAL FEVER CONCLUDED.

We must now continue the consideration of the two cases of fever mentioned in the last lecture. On the 23d, S. J. was relieved so far, that she was free from delirium and got sleep in the night, but still had hot skin, headache, pulse 120, and complained of thirst, and of her throat being sore; the bowels were too much purged, and therefore I left off the mixture, and ordered her

Sulph. quininæ gr. i;
Acidi sulph. dil. ♏j, *ex*
Aquæ ℥j *ter die.*

On the 24th she said she felt better, her head nearly clear, and had had some sleep in the night; the skin continued hot, the pulse still harder than natural, and the tongue furred; the bowels continued to be relaxed enough, four or five times in twenty-four hours, and therefore I made no alteration in the medicine, except the addition of gr. j *ipecacuan.* to each dose as an expectorant, as the cough continued; and as there remained some tendency to flushing, and heat in the head, she had *eight leeches to the temples.* The *ipecacuanha* produced some nausea and expectoration; her pulse fell on the 26th to 108, and soft; skin more temperate; the bowels continued relaxed. The report yesterday, the 27th, was, that she felt much better; pulse 96, soft, and full; coughs still, but expectoration easy, mucose; tongue moist to-day; she may almost be pronounced convalescent.*

M. H. continued to suffer from pectoral and gastric inflammation, notwithstanding the active and antiphlogistic measures adopted; on the 22d she had *six more leeches* applied to the *sternum*, which removed the pain, but the dyspnœa and cough continued. On the 23d she had less fever, little or no headache, and the skin soft; she was very much purged, but no involuntary motions, and I *omitted the antimonial mixture*, and gave her the *emulsio oleosa* for her cough. On the 23d, nine p. m., as the purging continued, Mr. Williams thought it necessary to give ♏x *tinct. opii post sing. sedes liq.* After the second dose she fell asleep, and on the 24th she felt better in the morning in consequence of having had rest, but in the afternoon became worse, the crepitation continuing, with dyspnœa, and the cough sounding wheezy. She had *decoct. hæmatoxyli* for the bowels, with gr. ij *ipecacuan.* as an expectorant, *6tis horis*; *eight leeches* and a *blister to the chest*; next day she felt better,

* The convalescence was gradually progressive from that time; the patient subsequently had only some emmenagogue and tonic medicines.—*Rep.*

and on the 26th also, though she had been delirious in the night, and her breathing was oppressed at times, although the respiratory murmur was better. On the 27th, yesterday, she was much worse again; she had been delirious and restless in the night, getting out of bed; she was at two p. m. incoherent, talking anxiously about members of her family; the respiratory murmur became weaker again on the left side, with crepitation, and respirations more frequent, besides which she now and then gave a very faint suppressed cry, as if something gave her pain, though she was too confused to give any account of what it was, but there could be no doubt from the symptoms that there was a renewal of pleurisy, or rather pleuroperipneumony. Under these circumstances, though she was much reduced, and the pulse was becoming weak, it was necessary to *take some blood* from her, which we did to the amount of ℥xvij, upon which the pulse became rather fuller, and she less restless. I directed her to have the *head shaved*, and ordered her *decocti cinchonæ* ℥j *ter die*, for her skin was temperate, and though the vessels required unloading, the constitution needed support, and she was constantly supplied with *milk*. To-day the report is very favourable, though she can scarcely be said to be out of danger; she was very restless in the early part of the night, but slept afterwards, and is drowsy to-day; not comatose, being collected when awakened. Says she feels hot all over; pulse 108, not weak; tongue moist on the edges; bowels free; urine natural; skin temperate; does not cough frequently as before.

If she has no return or exacerbation of pleuro-peripneumony she may do very well, as the fever is gone.* I must again impress upon you the necessity for giving nourishment, and not merely contenting yourselves with ordinary medicines in fever, when patients are in a state of insensibility, at which time they are not conscious of thirst, though standing much in need of the support of drink, of which you will be convinced by seeing the greedy thirst of patients just recovering their sensibility in fever; if you do not insist upon the attendants giving supplies of liquid at this stage, they will, perhaps, let the patients sink, from their not merely not asking for, but refusing it, and you must watch your opportunity to get them to take a sufficient quantity at a time, from a quarter to half a pint, as during a period of insensibility it is difficult to get them to swallow; and, besides, you must caution attendants against putting liquid into the mouth of a patient lying down insensible, as I have no doubt in this way medicine or nutriment has occasionally become the cause of death rattle, by getting into the trachea. When attendants tell you patients will not, or cannot, swallow a spoonful, you may, nevertheless, get them to drink a teacup full, by setting them up, and rousing them a little, which though not desirable so far as the fever is concerned, must be done to prevent inanition. Whilst the fever lasts, I never wish my patient to drink anything but spring water or milk, and you will find these most uniformly grateful; water for thirst, milk for nourishment. Most generally the smell of food, particularly meat, is nauseous to fevered patients; and hence I do not order beef-tea or broth; milk is at the same time the simplest and best nourishment for all ages.

* From this day she continued to get better. On the 31st the cough was "slight and loose," and she quickly recovered her strength. There was no alteration of her medicine after the 27th.—*Rep.*

DISEASES OF THE HEART.

Both the cases of disease of the heart, which I explained to you a fortnight ago, are improving. T. M. (p. 330) in particular has every prospect of getting permanently well; he continues to take the digitalis and opening medicine; his pulse is now 80, and though his breathing is still easily affected by exercise, he is stronger.*

The other man, J. W., has made some progress, but is still suffering from rheumatic pains, though less; he has been taking gr. v *sulphate of quinine*, with ℥ss *vini colchici ter die* since the 20th; he has still a difficulty of lying on the left side; bruit de soufflet, and the other symptoms of the organic disease of his heart, for which we must keep him at rest as long as we can.†

* He was discharged from the hospital on the 7th of February, "feeling quite well; no dyspnœa or impulsion on ascending the stairs; pulse 81, full, and steady."—*Rep.*

† This patient was permitted to leave the hospital at his own request on the 5th of March as he "felt well; pulse 88, regular, soft, still jarring; no dyspnœa or pain in the chest, but bruit de soufflet still, and impulsion upon exercise."—*Rep.*

CONCLUDING REMARKS TO THE COURSE.

I have now, Gentlemen, come to the conclusion of my three months of clinical lectures, but although I vacate the chair, I shall continue to give you less formal, though, perhaps, not less useful instruction, both in the wards, and in pathological examinations, when opportunities occur. Indeed part of the clinical lecture must necessarily be a recapitulation of peripatetic observations, but those observations are sometimes not heard. I recommend you carefully to attend to the practice of the other physicians, as well as the constitutional treat-

ment adopted by the surgeons. I can assure you, that you will glean something from the practice of each and every one, and it is only thus that you can learn to be what is of so much consequence, unprejudiced. I have heard teachers recommend pupils not to wander from one professor to another, for that thus their notions would be unsettled, and not regulated by any system. But as no system which is the work of man can be perfect, I advise you to learn from one professor to correct the system of another, and so far as you have time and means, to visit other schools, both in this country and abroad.

You will do well to attend constantly to the treatment of the out-patients, as you can thereby learn the nature of many chronic diseases, especially those of the eye and skin, which are of much consequence though not generally requiring admission into the wards. As far as our time would allow, I have shown and explained to you our new and inestimable mode of diagnosis by auscultation. I feel that to the practical physician it is like the acquisition of a new sense; but this you must practise yourselves, constantly, in order to render perception clear. Let me, however, remind you, that its value is in distinguishing the disease, and that though, according to the proverb, the knowledge of the disease is half the cure, it is but half, for if you keep your ear to the stethoscope for a week, it cannot whisper to you what remedies to resort to; therefore, practise auscultation and morbid anatomy to ascertain the nature of diseases; but withal, watch well the effects of remedies in order to cure them. I have endeavoured to teach you the true value of morbid anatomy,—to show you, that though some diseases are incurable, you may yet prolong life, and that you will do so more certainly, by knowing that the disease can only be palliated, than if, from not being assured of that, you should attempt impossibilities; and this applies more especially to organic alteration of the heart.

There are some men who, through ignorance, from want of being better taught, underrate morbid anatomy; I say through ignorance, for I hope there is not any medical man so devoid of principle as to neglect it if he knew the value of it as much as we do. It is true that there are some diseases connected with a derangement of the nervous system, the immediate cause of which has not yet been satisfactorily detected by dissection; but we shall succeed in time, I believe; and the phrenological mode of unravelling the brain must at length lead to discoveries of the nature of several obscure diseases. It is of the greatest consequence to you, in order to gain confidence and steadiness in the application of remedies, to lose no opportunity of post-mortem examinations, and especially in acute diseases, such as fevers and inflammation of the viscera. A knowledge of the true nature of diseases gained by morbid anatomy, will give you confidence to adopt decided practice; knowing that if the disease be within the power of remedies, you will cure, and if the disease be mortal, you will not hasten death a moment; on the contrary, perhaps, gain time for the sufferer to settle his affairs, which is often of the utmost consequence to a family.

In order to instruct you in morbid anatomy, I have not confined myself to the specimens obtained actually from our own clinical patients during the session, but I have also resorted to the museum for exemplification, and have brought forward specimens obtained out of doors, with which I have been favoured by friends, including former pupils, to whom I feel much obliged. Whenever I receive any specimen, or letter communicating medical facts, from a former pupil, it is a source of gratification, as a proof of that good understanding and friendship which ought to exist when each party does his duty; I receive it as a proof of conviction, that what is thus communicated will not be thrown away, and that there is a feeling of agreeable recollection of our school, and that my old pupil wishes to help me in instructing my new ones, that he does not consider me in his debt, or that I had broken faith with him in any way, but that, on the contrary, I had given him value for what he paid according to the custom of the school,—that he has that feeling which makes him wish us prosperity, as I do most cordially to all of you, Gentlemen, in taking leave of this class for the present.

THE EXPERIMENTS
OF
FARADAY, NOBILI, AND ANTINORI,
ON A NEW CLASS OF
ELECTRO-DYNAMIC PHENOMENA.

The late discoveries of our distinguished countryman Dr. Faraday in this important branch of physics, has excited the attention of natural philosophers in every part of Europe, and his experiments have been successfully repeated and modified by many of his contemporary savans. Amongst other exotic inquirers, Signori Nobili and Antinori of Florence, who derived their first knowledge of Dr. Faraday's pursuits from a short notice given by M. Hachette to the Academy of Sciences at Paris, and after-

wards published in "Le Temps" journal, are the most distinguished. The researches of Nobili and Antinori were published in the Antologia, No. 131, and were translated into the Philosophical Magazine of June last. This paper has been extracted by Dr. Faraday, and printed, with annotations, for private circulation, but as the reputation of this justly eminent chemist is now a sort of national property, we shall make no apology for devoting a portion of our pages in justification of his claims to these important discoveries. It has been said, and the assertion has been industriously spread by certain *liberal* and *candid* persons, that the two Italian physicians were the originators of the inquiry, but we are happy to observe, that those gentlemen do justice to the merit of our countryman, and frankly declare, that their "particular opinions do not in any way diminish the intrinsic merit of Mr. Faraday's discovery. It is one of the most beautiful of our time, whether it be considered in itself for the largeness of the vacancy it serves to fill, or for the light it throws over the various theories, and especially that of the magnetism of rotation." The following is the notice which appeared in "Le Temps," and which being translated into several languages, has served as the text of all subsequent papers on magnetic electricity.

"The memoir of Mr. Faraday is divided into four parts. In the first, entitled Induction of Voltaic Electricity, is found the following important fact, that a voltaic current which traverses a metallic wire, produces another current in a neighbouring wire; that the second current is in a direction contrary to the first, and continues but for a moment; that if the producing current is removed, a second current is manifested in the wire submitted to its action, contrary to that which was first formed in it, i. e. in the same direction as the producing current.

"The second part of the memoir treats of electric currents produced by the magnets. On causing helices to approach to magnets, Mr. Faraday has produced electric currents; on removing the spirals, currents in the contrary direction were formed. These currents act powerfully on the galvanometer; pass, though feebly, through brine and other solutions, and in a particular case, Mr. Faraday *has obtained a spark*. Hence it follows, that this philosopher has, by using a magnet only, produced the electric currents contemplated by M. Ampere.

"The third part of the memoir is relative to a particular electric state, which Mr. Faraday calls *electro*-tonic *state*." [Vide LANCET, Nos. 456, 457, 458.]

"The fourth part speaks of the experiment, not less curious than extraordinary, of M. Arago, which consists, as is known, in making a magnetic needle revolve under the influence of a rotatory metallic disc, and *vice versa*. Mr. Faraday considers this phenomenon as intimately connected with that of the magnetic rotation, which he had the good fortune to discover about ten years ago. He has ascertained, that by the rotation of the metallic disc under the influence of a magnet, there may be formed electric currents in the direction of the rays of the disc, in sufficient number to render the disc a new electrical machine."

The Signori proceeded to observe, "We had no occasion to make trials before we succeeded in the experiments of Mr. Faraday. The first spirals which we brought near the pole of a magnet, quickly manifested their influence on the galvanometer. We observed three facts in succession. Whilst approaching the magnet, the needle of the instrument is, in the first place, seen to deviate a certain number of degrees, which indicates a current excited by the magnetism, in the spirals previously made to communicate with the galvanometer. This current lasts but for a moment, and is then completely extinct, as is proved by the needle returning to its first position. This is the second observation. The third (finally) occurs when the spiral is taken from the magnet: the needle of the galvanometer then deviates on the other side, demonstrating the development of a current contrary to that excited in the first instance.

"On experimenting with an annular spiral between the poles of a horse-shoe magnet, we observed that the action was much less than that produced with the same spiral when the lifter of the magnet was put to it or suddenly taken from it. This fact suggested the idea of rolling a copper wire, covered with silk, round such a magnet, so as to have an apparatus always mounted for the experiment in question. The spiral tube subjected to the magnetic influence is then always upon the magnet, and the immediate cause of the phenomena resides in the lifter, because of the property which that little piece of soft iron possesses, of being magnetized and demagnetized rapidly. When the lifter is detached, the spiral, which, before, was, in the presence of this piece of iron, strongly magnetized, is suddenly removed from its action, and represents the case of a spiral, which having been first approximated to a magnet, is then removed. When the lifter is replaced, it is as if a magnet were caused to approach the spiral, for the lifter becomes magnetic on being attached to the poles of its own magnet.

"This arrangement, besides being very

active, has the advantage of supplying the philosopher with a *constant source* of voltaic electricity. The want of a constant current is often felt in such researches; and if thermo-magnetism offers a plausible means of satisfying such necessities as I have indicated elsewhere ('Ann. de Chimie,' Feb. 1830, p. 130), yet the new method offered us by a magnet covered with electro-dynamic spirals, is not to be despised. Here the currents are always ready to be manifested. Suppose, as is usual, the lifter of the magnet is in its place, nothing more is required to obtain a current in the spiral than to detach the lifter, the current in the wire being, as it were, at first in a latent state.

"There are two modes of using this arrangement: the one by attaching the lifter, the other by detaching it. When the two motions are made with the same rapidity, and with relation to the same points of the magnet, the deviations are in the inverse directions to each other, but precisely of the same value. The detachments are, however, always equally instantaneous, and for constancy of effect are preferable to approximations; for the latter, to be always equally successful, would require a mechanical arrangement, which it is not worth while either to imagine or execute. By taking care that the lifter is constantly in its right place and position, there will always be produced the same deviation of the galvanometer when it is detached from the magnet. This, we repeat, is a valuable result applicable in numerous cases, and perhaps proper to measure the force of large magnets in a more exact manner than by the ordinary mode of ascertaining the weights sustained.

"The arrangement described is highly advantageous; but does it produce the maximum of electro-dynamic effects? There is indeed another, much better, which consists in applying the electro-dynamic spiral to the central part of the lifter, corresponding to the internal, which separates the poles of the horse-shoe magnet. In this position a spiral of a few turns is able to surpass the effects of a far greater number of spirals otherwise disposed. The central part of the lifter is to be entirely covered with wire, leaving exposed only the extremities, which are to come in contact with the pole of the magnet. The ordinary form of the lifter is not the most convenient upon which to arrange this species of electro-dynamic ring, but upon conveniently modifying its shape, the wire may be applied with facility, and thus the effect be obtained at its highest degree of intensity. The reason is evident; for two conditions, in fact, require to be fulfilled: one, that the spiral should be subjected to all the influence of the magnetic force; the other, that this influence should be abstracted in the shortest possible time. Now the wire round the lifter is exactly in the most favourable position for the magnetic force to be concentrated upon it; and this force vanishes the instant the lifter is detached, as is required by the second condition."

Hitherto these philosophers have been anticipated in all their experiments and conclusions by Dr. Faraday. They next inquire into the affections of certain metals as respects the new electro-dynamic principle.

"SPIRALS OF VARIOUS METALS.

"The metals with which we have experimented are four, *copper, iron, bismuth, antimony*. Iron is interesting as the foremost amongst magnetic metals; bismuth and antimony for the distinct position they hold in the thermo-magnetic scale. In experiments made under circumstances approximating to equality, it appeared that copper was the most active in the present point of view; then, at a little distance, iron; afterwards antimony; and, finally, bismuth."

These gentlemen preceded Dr. Faraday in the discovery of the electric spark. That gentleman alludes to this circumstance in the following terms. "Being much engaged in the investigation and confirmation of the laws of magneto-electric action, terrestrial magnetic induction, &c. &c., some of the results of which are contained in my second paper it, will be seen that in the race which Sig. Nobili and Antinori probably inadvertently ran against me, they obtained the electric spark from the common magnet before me. I have great pleasure in bearing witness to the accuracy of their reasoning on this point, and also to the success of the result. Having made a variation of the experiment, by obtaining the spark from the action of a common loadstone, in which their most perfect mode could not be applied, I will take the opportunity of describing the simple adjustment I have devised. (Vide LANCET, No. 457.) A helix was fixed round the lifter, the wire ends were raised upwards; one which may be called *a*, was bent into a hook; the other *b*, after rising, was bent at a right angle, and had a thick, small, circular plate of copper fixed to it, which was made by the spring of the wire to press in the middle slightly against the rounded end of *a*; this plate and the end of *a* were amalgamated. On bringing the lifter down suddenly on the poles, the momentum of the plate caused it to separate from the end of *a*, and the spark passed. On lifting it up the concussion always separates the end of *a* from the

plate, and a spark is again seen. When the plate and point are well amalgamated, the spark will not fail once in a hundred times, either at making or breaking contact. I have shown it brilliantly to two or three hundred persons at once, and over all parts of the theatre of the Royal Institution.

"As Professor Richie expresses it, the spark has not yet been obtained, except from a temporary magnet, i. e., from a magnet in the act of being made or destroyed. I obtained the first spark from a soft iron magnet, made by the well-known influence of electric currents. Sig. Nobili and Antinori obtained the second spark from a soft-iron magnet, made so by the influence of a common artificial steel magnet: their result has been repeated by a great number of persons. Mr. Forbes of Edinburgh first obtained the spark from a soft-iron magnet, made so by the influence of the natural load-stone. The latter experiment is also that which I have made with Mr. Daniell's load-stone, lifting only about thirty pounds, and in the manner described. I was not aware of any other modes of performing the experiment, except my original one, and Sig. Nobili and Antinori's."

The following are the observations of the Florentine philosophers on this point:—

"The voltaic pile gives a spark, only when composed of a certain number of pair of plates. A single Wollaston's voltaic element yields it; and when of a certain activity, produces it constantly at the surface of mercury, to which the conjoining wires destined to close the circuit are conducted. In the voltaic pile having a certain degree of *electric tension*, the sparks pass between the zinc and copper poles, either in the case of opening or closing the circuit. In a single Wollaston's element the tension is feeble, and the spark occurs only when the circuit is interrupted. At that moment the current, which before was moving, accumulates, as it were, at the place of interruption, and acquires the intensity necessary to cause the spark. Such tension is wanting in the other case of closing the circuit, and the spark also is absent." The current developed in the electro-dynamic spirals by virtue of "magnetism, are also in motion, but circulate only for the moment during which they are approaching to or receding from the magnet. It was, therefore, we concluded, in one of those two moments, that we ought to open the circuit in making the experiment for the spark."

The improved plan which these gentlemen devised to secure this beautiful result at pleasure, was this:—

"The whole of the contrivance is attached to the lifter of the magnet. This piece, which is a parallelopiped, is surrounded in the middle by the electro-dynamic spiral, to which it is firmly attached by two pieces of brass, so that the latter can enter between the magnetic poles, whilst the lifter comes in contact with the poles in the ordinary way. The extremities of the spiral come in contact, one with each magnetic pole, by means of two little springs in the form of wings attached to the lifter, and which press lightly against the poles when in its place. To leave room for these springs, the lifter is narrower than usual, covering about half the poles; the remaining space serves for the contact of the springs, which are in this way isolated, as it were, from the lifter; and yet by means of the magnet itself, serve to complete the electro-dynamic circuit. Suppose that the lifter is in its place, the springs touch the poles, and the circuit of the spirals is metallically closed by the magnets. On detaching the lifter, the circuit opens in two places, and either at the one or the other interruption, the spark almost constantly appears. When the effect does not take place, it is because the separation has not been well effected; but it is so easy to repeat the experiment, that it is useless to think of a piece of mechanism to remedy an inconvenience which is so readily obviated.

"In this apparatus the spiral on the lifter was of *copper*. On substituting an *iron* wire, the spark also occurred. This experiment was interesting in illustration of an influence which the ordinary power of the magnet over iron, might exert upon the electro-dynamic influence. It did not appear that the one action disturbed the other."

The next essays of Nobili and his confrère were directed to the subject of *terrestrial magnetism*.

"We took a paper tube two inches in diameter and four inches long; a copper wire 40 metres long was coiled round it, the two ends being left at liberty to connect with the galvanometer; the tube was trimmed at the ends, so that it could be placed upright upon the table either in one direction or the other, at pleasure. A cylinder of soft iron, as is well known, placed parallel to the dip, is subject to the terrestrial magnetic influence; the lower part becomes a north pole, the upper a south pole. This is a phenomenon of position always occurring in the same direction with this kind of iron, which is as incapable of retaining the magnetism received, as it is disposed to receive the new magnetism to which it may be subjected.

"In our latitude, the inclination of the needle is about 63°. The paper tube with its spiral was therefore arranged in that direction, and an iron cylinder introduced. Whilst in the act of introducing it, the galvanometer was seen to move, owing to the

presence of an electric current excited by the magnetism. On taking out the cylinder the motion was reversed; there is no doubt therefore, that terrestrial magnetism is sufficient of itself to develop currents of electricity. It should not be concealed here, that in the above experiment the electricity is developed by the intermedium of soft iron introduced into the spiral. This, without doubt, is true, but it is also true that it is not essentially necessary to recur to this aid to obtain unequivocal signs of the influence of which we speak. On placing our cylindrical spiral so that its axis should be parallel to the magnetic dip, and then inverting it by a half revolution in the magnetic meridian, we observed at the comparative galvanometer the signs of a current excited in the spiral by the *sole influence* of terrestrial magnetism. It is not even necessary, for this effect, to place the spiral in the direction of the dip. The experiment will succeed in the vertical position; the effect is less, but always so distinct as to remove every error. We experimented with three copper wires of different diameters; the smallest was 0. 5, the second 0. 66, and the third 1., millimetre in diameter. The effects increased with the size: the first gave deviations from 2 to 4, the second from 4 to 8, and the third from 10 to 20."

The trials of these experimentalists to develop the properties of the electro-tonic state were unsuccessful, but they propose returning to the consideration of the question with more efficacious means, and to extend their research to thermo-electric combinations, which deserve to be studied in the same point of view, as they have never yet presented sensible signs of electric tension. Touching the chemical and physiological effects they observe:—

"The new currents of Faraday pass, although with difficulty, through humid conductors; that such is the fact, may be readily verified by introducing a conductor of that kind into the circuit of the electro-dynamic spiral. In the case of other known currents, I have demonstrated elsewhere, that there is always chemical decomposition when they pass liquid conductors; and that, however feeble they may be, the decomposition is always assured by their transit through the fluid. It is, therefore, very probable, that the new currents will produce the phenomena of decomposition, but their distinctive character of brief duration must not be forgotten. I believe that the time, however short, is still sufficient for decomposition."*

"*The physiological effects* consist, as is well known, in the shocks or contractions of the muscles, the acrid and acidulary taste on the tongue, and the light before the eyes. For obtaining these effects, it is absolutely necessary that the electricity should penetrate into our organs,—these latter belonging to humid conductors. This path, as we have seen, is very difficult for the new currents; nevertheless the frog put into the circuit of our electro-dynamic spirals, arranged around the lifter of our magnet, was powerfully convulsed each time that the lifter was separated or attached. The experiment is beautiful and instructive; beautiful because of the energetic convulsions produced, apparently by the immediate action of the magnet, and instructive because it confirms the fact of the passage of these currents through humid conductors, and because also it shows that the frog is in all cases the most delicate galvanoscope. The frog properly prepared is sufficient for the purpose of arriving at the knowledge of thermo-electric currents, and would have been quite adequate to discover the new currents of Faraday. Although it is not by this road these discoveries have been arrived at, still it is not less true that they might have been made by the simple assistance of this interpreter, which astonished Europe in the first times of galvanism."

Dr. Farnday likewise obtained the effects of the sensation on the tongue, and of light before the eyes. Nobili continues his inquiries into the "magnetism of rotation." "In the experiment of M. Arago we have magnetic poles in presence of a disc, which, instead of being quiescent, is continually moving on its own axis. The latter condition is the only one added, and by it we see that the final result of the phenomena will be excessively complicated, but that in reality nothing new will happen. In all cases it is the currents developed by the magnetism at the place of the disc that is directly acted upon by this magnetism, which are concerned. This part is rapidly removed by the rotation, and another comes forward which is subjected to the same influence, which always tends to form currents in the contrary direction to those which may be supposed to exist in the magnetic pole. These currents, by their nature, tend to be inverted so soon as they are withdrawn from the presence of the cause which produced them, and are, in fact, inverted every time that the velocity of rotation will permit it. The theory of this species of magnetism appears mature. The character which distinguishes this, does not consist in momentary duration only, which it has in common with soft iron, but also in being a double magnetism, *inverse* and *direct*; inverse at the moment of its production, opposite to the producing

* Nobili here speaks of his own researches, published in Bib. Univ. xxxvii, 10.

cause; *direct* at the moment after, when this cause disappears."

Nobili and Antinori appear to have mistaken the sense of Dr. Faraday's letter to M. Hachette. They proceed thus:—"Mr. Faraday considers Arago's magnetism of rotation as entirely connected with a phenomenon which he discovered about ten years ago." "*He then ascertained that by the rotation of a metallic disc under the influence of a magnet, there may be formed, in the direction of the radii of that disc, electric currents in sufficient number to render the disc a new electric machine.*" "We are quite ignorant how Mr. Faraday has ascertained the fact; we do not know "how a result of such a nature could remain so long a time generally unknown, and, as it were, lost, in the hands of the author of the discovery. Besides, there is here something very problematical to us, and before we leave the subject we will describe the experiment we made relative to it.

"A disc of copper was revolved, and two long copper wires prepared, attached at one set of ends to the galvanometer, and, at the other, held by the hand against the disc, the one at the centre, and the other at the circumference, in the direction of the radii. In the rotation of the disc, the points of copper pressed against it will be heated, but unequally; that pressed against the circumference will be most heated, and that at the centre the least. This difference is quite sufficient to determine an electric current capable of moving the needle of the galvanometer, and retaining it after a few vibrations at a certain degree of the division. When the needle is thus quiescent, if a horse-shoe magnet be advanced towards the plate so as to embrace it without interrupting its motion, it will be seen that the deviation of the needle will augment or diminish according as the poles act in the one direction or the other. The effect is a sure proof of the current manifested in the disc by the action of the magnet; but because the wires connected with the galvanometer are arranged with their ends in the direction of the radius of the disc, are we to conclude that they are exactly in the direction in which the current excited by the magnetism exists? We do not believe it, and though we should, with Mr. Faraday, admit this species of irradiating currents, there would still exist for us a great difference between this mode of exciting electricity, and the ordinary one of our common electrical machines. There is here a great void to fill, in passing from a superlative conductor, like the metallic disc of M. Arago, to the worst, such as the glass plate of an ordinary machine."

The words of M. Hachette, in the French translation of Dr. Faraday's letter in "Le Lycée," No. 35, are these:—"M. Faraday considère le phénomène qui se manifeste dans cette expérience, comme intimement lié à celui de la rotation magnetique qu'il a eu le bonheur de trouver il y a dix ans. Il a reconnu que par la rotation du disc metallique," &c. &c. The phrase used by Nobili and Antinori, in the body of their paper, is "Egli recourbe fin d'allora che," &c., whilst that which they used at the head of the paper, is "Egli ha reconosciuto che," &c. &c. It was in consequence of the recent researches that Dr. Faraday ascertained the state of the revolving plate, and could then refer the effects in its kind to that which he had so long before discovered. The experiments and speculations of the Italian philosophers must be acknowledged to possess the highest interest, but they are strikingly inferior in point of precision and severity of abstraction, to those of our talented countryman, to whose labours in the evolution of terrestrial electro-magnetism we shall shortly recur.

THE TREATMENT OF CHOLERA

By the Injection of Saline Solutions into the Veins, not warranted by the Pathological Condition of the Blood.

By James M'Cabe, *M.D., Cheltenham.*

Of the several modes of treating cholera morbus which have from time to time occupied the attention of the medical profession and of the public, the treatment by the injection of saline solutions into the veins, appears to have attracted most the attention of the profession. Whether a remedy be found out by fortunate chance or scientific deduction, the medical faculty are equally bound to avail themselves of the discovery; but a remedy laying claim to scientific foundation, is sure to meet with greater favour, and is more likely to be adopted without sufficient inquiry, than that which originates in mere experiment. Having premised these general observations, let us now inquire how far the treatment by saline injections into the veins is warranted by, or deducible from, the pathological condition of the blood in cholera.

Chemical analysis has shown that, in the blood of patients labouring under cholera, there is a deficiency of its watery and saline parts; and Dr. Latta states, that an article published in The Lancet by Dr. O'Shaughnessy, in which this pathological fact is stated, first suggested to him the idea of treating the disease by restoring to the blood its serum and saline particles. This he first attempted to do by

saline solutions given by the mouth, and also introduced by the rectum; but finding this plan unsuccessful, he had recourse to the more direct means of injecting it into the veins.

We find in all the analyses that have been published on the subject, that the deficiency of the watery and saline parts of the blood, in cholera, is referred to the profuse discharges which had previously taken place from the stomach and bowels. Now these discharges from the blood must be occasioned by some morbid condition of the system; and we can scarcely suppose that that morbid condition can be remedied by merely replacing by injection into the veins what that morbid condition had allowed, or probably had caused, to be effused. If we adopt, in our explanation, the humoral pathology, and suppose the cause of these discharges to be the state or condition of the blood itself, we must also adopt the language of the older physiologists, and refer it to an acrimony of the blood and humours. What this acrimony can be, unless it be an excess of saline particles in the blood, it is difficult to imagine; and if so, and if the discharges are occasioned by the vellication of the coats of the arterial exhalents by the saline spiculæ of the blood, it is evident that saline injections would be pathologically contra-indicated. If, on the other hand, we refer the immediate cause of these discharges to the pathological condition of the solids—that is, to the state of the containing vessels—we meet with the same difficulty; for we cannot expect to remedy the relaxed state of the solids by merely refilling the vessels with a fluid analogous to that which they had already effused.

It is, therefore, evident, that in founding the treatment by saline injections on the pathological condition of the blood in cholera, an effect has been taken for a cause, and made the basis of a practical deduction. The loss of the watery and saline parts is the effect of some antecedent cause, over which the return of them again into the system by injection can exert no remedial power. Could it be shown that an excess of salts in the blood was the cause of the profuse discharges, the injection of pure water, of the temperature of the blood, into the veins, would probably be beneficial by diluting the thickened crassamentum; but if the effusion from the exhalent vessels be the effect of diminished vitality of the solids, no benefit would be likely to result from any injection by the veins, as it would pass through the same channels.

There are many circumstances in the history of cholera that would favour the opinion, that an excess, and not a deficiency, of the watery and saline parts of the blood, is a predisposing cause of the disease. In the first place it attacks most frequently the poor and ill-fed portion of the population, more especially those whose constitutions are broken down by disease or intemperance;—individuals labouring under organic obstructions, and in whom dropsical effusions might be expected to occur, from which, in fact, the discharges in cholera differ only with respect to the situation in which the effusion takes place; for were the fluid which in cholera is poured into the alimentary tube, to be effused into the cavity of the abdomen, or cavities of the pleuræ, it would constitute ascites or hydrothorax. We know that the frequent use of aperient medicines, whether drastic or saline, has the effect of draining off, through the mucous membrane of the bowels, a portion of the watery and saline parts of the blood. We know that among the better classes of society in this country, considerable attention is paid to the state of the bowels; in fact, that aperient medicines are frequently taken, and consequently a considerable part of the watery and saline constituents of the blood removed in that way; while among the poorer classes, less attention is paid in this particular. We know that the French, generally, as a nation, are very much averse to the use of aperient medicines, and in fact seldom or never take any. In France the cholera has attacked the rich and the poor indiscriminately;—in this country its ravages hitherto have been very much confined to the poor. In France, the rich and the poor systematically and intentionally avoid taking aperient medicines;—in this country, the poor generally neglect to take them. In the progress of cholera hitherto through both countries, are we merely to consider it as a singular coincidence, or are we to view it in the relation of cause and effect, that the disease has principally attacked those persons who habitually or systematically neglect the use of aperient medicines?

From the latest reports published on the subject of the saline injections, it would appear that excruciating pain frequently follows the use of the injection, particularly when the fluid is again making its way into the stomach and intestinal tube.

From the various facts and circumstances here stated, and the inferences which appear to me to be fairly deducible from them, I am of opinion that there is no deficiency, but probably an excess, of the watery and saline parts of the blood in cholera, previous to the discharges which accompany the disease; and, consequently, that the treatment by saline injections into the veins, if found successful, ought to be referred to fortunate experiment, and not to the pathological condition of the blood.

UNSUCCESSFUL TRIAL OF

DR. STEVENS' SALINE TREATMENT

IN MALIGNANT CHOLERA.

To the Secretary of the CENTRAL BOARD OF HEALTH.

Rochester, July 21, 1832.

SIR,—Being strongly impressed with the hope that the chemical pathology of the blood, as shown in Dr. O'Shaughnessy's able analysis, would lead to a more rational and certain mode of treatment in this disease than any hitherto laid down; and, at that time being strongly excited by the simple mode of treatment recommended by Dr. Stevens (on this principle), and lauded by him with such praise and success in his practice in the Coldbath-Fields Prison, was determined to give this remedy a fair and unbiassed trial on the first opportunity that should occur.

An opportunity soon presented itself, by my having charge of an hospital ship attached to the convict establishment at this port, where the cholera had assumed a very malignant character.

Three cases were selected for trial, of the admissions during one day.

Case 1.—The medicine was given every hour, as recommended by Dr. Stevens, together with dry heat, frictions, mustard poultices, and injections of hot salt and water; but notwithstanding the most unremitting attention of myself and assistants to the faithful exhibition of this remedy, he died in about twenty hours after admission, or after he began to use the medicine.

Case 2.—Used the same remedy as in Case 1, immediately upon admission; died forty-eight hours after he commenced to take the medicine. Neither of these cases showed the least symptoms towards reaction during the whole period under treatment.

Case 3.—Was likewise put under the same mode of treatment as the two preceding cases, but observing them sink so rapidly under the saline treatment, and believing, from much experience in this disease, both in India and in England, that I had superior remedial powers at command, the saline treatment was accordingly laid aside. A mustard emetic was immediately given; bleeding by leeches from the region of the heart and præcordia; ten-grain doses of calomel, five of capsicum, given every hour; the mustard sinapisms, dry heat, and a diffusive stimuli, given as occasion pointed out, with a table-spoonful of cold water, as the craving for drink was excessive. The salt-water enema was continued, with an addition of ℥j of the ol. terebinthinæ to each; by which treatment reaction was soon established, and he is now at duty.

From my own practice and experience, and in that of some of my friends, of the saline treatment, I have no hesitation in stating my opinion that it is a remedy, *per se*, totally inert in the collapsed stage of cholera.

I can, however, state, that the hot saline enemata, when combined with the ol. terebinthinæ and the mustard poultices, as recommended by Dr. Stevens, are most valuable remedies in the stage of collapse from cholera. I have the honour to be, Sir,

Yours obediently, &c.,

JOHN ANDERSON, M.D.,
Surgeon, R.N.

Extract from another letter from Dr. Anderson, same date:—

During the last week I have had twelve boys in a complete state of collapse; they have all been treated under the plan pointed out, and I am happy to say that reaction has been established in all of them, with only one exception; and even this case, I trust, will still do well. It is rather singular, but true, that I have as yet lost no case where reaction has been once fairly established.

(Signed) JOHN ANDERSON.

BARBADOES NAPHTHA IN CHOLERA.

To the Editor of THE LANCET.

SIR,—May I be permitted through your talented publication to offer to the consideration of the medical profession, the probable advantages of employing Barbadoes Naphtha as a preventive against cholera, on the grounds of its being a compound simply of two elements, *hydrogen* and *carbon*, and containing a greater portion of *pure* carbon in a fluid state than any other known natural substance, the proportion of carbon being 85 per cent, whilst the greatest proportion of carbon in each of the subjoined active medicinal products, obtained from the most powerful vegetable tribes, is, "in brucine only 75 per cent, morphine 71, narcotine 68, emetine 64, and quinine 74." Thus demonstrating that the great proportion of carbon, in a soluble form, is the *principle* which accounts for the superiority of this highly curious product, over all other medicinal preparations, in particular cases, and its possessing the peculiar property of counteracting all morbid decompositions.

By a chemical analysis we are instructed, that a *drachm* of this natural product contains 52 grains of carbon, and which can be progressively increased to 1000 grains in

24 hours, whilst in an equal quantity of any other substance there are only four or five grains of carbon that could be safely exhibited, and which would be *inefficient in quantity* to counteract by its preventive powers the introduction of infectious miasma into the constitution.

I am, Sir, your obedient servant,
MEDICUS.

18th July, 1832.

REPLY OF MR. COSTELLO TO THE LETTERS PUBLISHED IN DEFENCE OF BARON HEURTELOUP'S NEW MODE OF BREAKING STONE IN THE BLADDER.

To the Editor of THE LANCET.

SIR,—In transmitting to you for publication in your valuable journal, a translation of my two letters to the President of the Academy of Sciences, I thought I had placed the object I had in view, namely, scientific and useful discussion, in such a clear light, that my motives could neither be misconstrued nor misrepresented. It is not my fault that it is so, and I sincerely regret it. In those letters I declared, as I do now, that I only sought for truth. I have no feeling, no object, in common with those who are interested "in making the worse appear the better reason." I have said then, as I repeat now, "Prove to me the value of your proposal, and I will freely acknowledge it;—show me the justness of your pretensions, and I will recognise them." There was surely nothing in this which could have caused it to be looked on as an improper demand. The truth must have been arrived at by the discussion into which it naturally led. There was nothing to call for a display of petulance or bad temper.

I am sorry that it was deemed advisable to assume so acrimonious a tone in replying to what are called my calumnies. No respectable member of our profession can applaud such a course. Respectful, is quite as effective as intemperate, language, to maintain the truth of our opinions. Both from feeling and habit I am averse to the use of harsh words, and I will not, therefore, allow myself to be betrayed into an imitation of the tone or spirit which breathes in some of the justificatory and reprobatory letters addressed to me in this week's LANCET, in behalf of M. Heurteloup. Angry controversy I will have nothing to do with, if I can avoid it. The discussion which sinks into vulgar abuse, and which employs no better arguments than offensive personalities, I cannot condescend to prolong. It is at once foreign to and beneath my object. With these observations, it would, perhaps, be right to discuss these letters altogether, and I am sure every respectable member of the profession would approve of this course. Still, the fear that some well-meaning persons might form an erroneous opinion of me from these misrepresentations, induces me to take the trouble of replying to them.

Before I come to the letters, I shall take leave to introduce a few remarks. Since the first establishment of lithotrity as a surgical operation by my colleague M. CIVIALE, many persons (and I say it without disrespect) have laboured for an easily-acquired celebrity in connexion with it, by alterations or additions made in the construction of the instruments. No person has toiled more sedulously for this purpose than M. Heurteloup. In the infancy of lithotrity, *brise-pierres* or stone-breakers, with two branches, were proposed, and sometimes used. M. Heurteloup produced one of these. Four-branched pincers were also used; of these M. Heurteloup produced a modification. He also proposed moveable or complex drills, the parent idea of which, with various models, was already in existence; and, lastly, the curved percussor, copied from Weiss's, or even an older instrument. Now be it remembered, these instruments followed each other in regular succession, and as regularly were they laid aside. The appearance of any one of them was always deemed a legitimate opportunity for some oblique reflections on Civiale's instruments. Despite of the good Civiale's did, and are still doing, they were always on those occasions sure to be reviled. Each of these instruments in its turn was lauded *as the "ne plus ultra" of surgical power in lithotrity*, and in its turn was supplanted by another specimen, borrowed, or indigenous of this author's fecundity. It has long been my intention to offer to the profession a reasoned critique on the mechanism and action of this system of instruments. I shall now do so with as little delay as possible. With regard to the curved percussor, I have some remarks to offer, which shall be communicated in a separate paper.

The first of those letters is from Mr. Edwards. It may be disposed of in a few words. It commences by an allusion to the postscript of a paper published in THE LANCET a little time before. This postscript was merely a propitiatory version of the mishap which occurred through the use of the percussor. In a note, he says, "It was a first essay, and therefore little better than a model" (strange admission). Since then, he adds, "Sixty-two have been made, each more perfect than its predecessor; and the weight of the hammer being determined,

and the probable resistance of the stone calculated, the whole strength of a man may be applied in striking, without any fear of the beak giving way." Mr. Edwards, it will be perceived, has no mean opinion of man's credulity.

A certificate from Messrs. Brodie and Hume is given with this letter. It is unnecessary to say anything more respecting this certificate, than that it should be placed at the head of my second letter to the Academy. It will be found that they say precisely the same thing.

Next is a letter from Mr. Goodwin, in which this gentleman very quietly accuses me of that of which he is himself guilty—namely, of making statements not founded in fact, and to which, in his regard for truth, he says he feels bound to offer a positive contradiction. Let us put this gentleman's love of truth to the test: he states, "that having heard that there was a safer mode of cure than the use of the knife, practised by two gentlemen in the profession, both strangers to me, I inquired about it, and was recommended by a friend* to Mr. Costello. Upon this circumstance being known to Mr. Brodie and Mr. Morrat, my medical attendants, they both, *for reasons best known to themselves, withdrew*, and I was left alone waiting the return of Mr. Costello to town."

There is an obliquity about this sentence evidently designed to favour the inference that Mr. Brodie would not meet me in consultation, from which an honourable mind recoils. What then must we think of Mr. Goodwin's statements, when this first of them is unjustifiable either in spirit or fact? I felt it my duty, on reading this paragraph, to call on Mr. Brodie. I pointed out the phrase, he read the whole letter. He said "I am at a loss to conceive how such an unwarrantable use could be made of my name. I saw Mr. Goodwin sometime before he sent for you. I told him what I had heard on former occasions, both from yourself and M. Heurteloup, that where there was paralysis of the bladder, it was not a favourable case for your operation. This I mentioned to him, and I heard nothing more about it till long after you had seen him. I saw him afterwards with M. Heurteloup, upon the invitation of Mr. Goodwin, which I should as readily have done with you, had I been invited. I will not be the partisan of any person." He added, "This seems to distress your mind, and you are to blame for it. I should take no notice whatever of these attacks, and you will find out, by-and-by, that they cannot in the slightest degree affect your success in your profession." When I experience such kindness and receive such advice from a gentleman so deservedly distinguished in science, I can well abate the anger which I may have first felt at Mr. Goodwin's ungenerous insinuation.

Mr. Goodwin states, that neither he nor Mr. Hodges ever saw any portions of stone voided—in fact, they believe that I never even felt the stone. Then, let me ask, what was it that was detached from within the bladder, and voided by the sound? What was it we caused to be dried on paper? And what was it that induced me to give him a hope of being relieved without the intervention of any serious operation, if it was not this sabulous or chalky matter which was detached and voided? It would have been singular, indeed, that I should have proposed to use Hunter's forceps to dislodge the stone, which, by the way, would have been as effective for that purpose as the percussor, if I had never felt the stone, and ascertained its position.

Mr. Goodwin further states that three sittings *of a quarter of an hour each or more*, and not two of five minutes, did take place. Out of thy own mouth shalt thou be convicted. The following is copied from a letter from Mr. Goodwin to me, after I had ceased to see him.

"Dear Sir,—Having *twice* borne the operation of the lithotrite, I trust I have given you a fair opportunity of exercising your skill with success, if mine was a case which came within the range of your instrument; but as I cannot bring myself to think so, or to undergo another trial, I take leave to say that I must decline doing so. I remain, dear Sir, &c.,

"GEORGE GOODWIN.

"Aug. 31."

Now perhaps Mr. Goodwin will be able to recollect which of us is right as to the number of attempts. What would Mr. Goodwin think of four unsuccessful attempts in one case, and five in another, which M. Heurteloup acknowledges that he made? With respect to the quarter of an hour's duration, I have only to observe that both M. Civiale and myself make it a rule never to prolong the sounding or lithotrising beyond from three to seven or eight minutes; we advise postponing the operation, rather than the bladder should be too much fatigued by sounding, for the state of the organ should always be respected. Now I have performed lithotrity in private practice as well as in public hospitals in England, before some thousand surgeons and others, and I defy any one to say that I have acted against this maxim in a single instance. I cannot admit, even for Mr. Goodwin's love of truth, that I departed from it in his case.

Mr. Goodwin is quite deceived, when he says I proposed to call in M. Civiale only.

* Mr. Bowdery, of Oxford Street, on whom I operated two years ago.—W. B. C.

When Mr. Goodwin declined this, I mentioned M. Heurteloup, and begged him to understand that I had no objection to his consulting him, that I wished not to be considered as standing in the way of his doing so.

At the second attempt, I proposed by means of Hunter's forceps to dislodge the stone, which I distinctly felt in the situation described, beneath the neck of the bladder, and from which it could not be removed, although the bladder contained a pint of injected water, and the position of the pelvis was raised, and varied from side to side. I left him to make up his mind on this matter; some days after, I received from him the above-mentioned letter.

Mr. Goodwin's gratitude to M. Heurteloup is a very proper feeling, I leave him to indulge it; his love of truth is quite another thing, and as it concerns me, I have taken the liberty of setting him right.

The next letter is from Mr. Briggs. It would be a waste of time to consider any communication on a subject of surgical science, emanating from a young gentleman, who has but just entered the profession. To animadvert seriously on anything which he pretends to utter of his own knowledge, would be breaking a fly upon the wheel. But this young gentleman is merely the cat's-paw in this matter; of this, there is sufficient internal evidence in the letter bearing his signature.

Before I come to the statements contained in this letter, I shall say a few words on his English. He begins, "The letter inserted by Mr. Costello in last week's LANCET, being evidently not written in a scientific point of view." As well might this unfledged controversialist talk of a letter written in a sun-beam; and again with risible solemnity, he says, "As Baron Heurteloup's pupil, *which* great advantage has enabled me to become acquainted with all that relates to his operations, &c." Make out who can the antecedent to *which?* it may be *Baron*, or *pupil*, or *As*, aspirated of course. But as I prefer an antagonist more worthy of me, I shall put you aside, Mr. Briggs, with a recommendation to go to school. Indeed you seem to want it; and notwithstanding you say you consider it your duty to take this upon yourself against your patron's wish, I shall persist in regarding your interference as both his work and his wish too.

After alluding to my letters to the Academy, with the flippancy congenial to ignorance, he asserts that those letters were never read in that learned assembly. With you, Master Briggs, I must deal decisively. I received a letter from the Secretary of the Academy, of which the following is a copy:—

"Institut de France, Academie Royale des Sciences, Paris le 23 Avril, 1832.

"Le Secrétaire perpetuel de l'Académie à M. Costello, à Londres.

"Monsieur, l'Académie a reçu la lettre que vous avez bien voulu lui adresser sur les nouveaux instrumens de Lithotritie de M. Heurteloup. J'ai l'honneur de vous prevenir, *qu'apres en avoir entendu la lecture*, l'Academie l'a renvoyée à l'examen de MM. Boyer, Dupuytren et Serres, commissaires déja nommés pour prononcer sur les instrumens du chirurgien Français.

"Agreez, Monsieur, l'assurance de ma consideration distinguée.

"(Signed) ARAGO."

My second letter was also read in the Academy at the sitting of the 18th June, of this he will find a proof in the *Messager* of the 1st instant. What think you now, Mr. Briggs, of the accuracy of your statements? If you understand French as well as you write English, and that will not be saying much for you, you will see that you have been misinformed with regard to the reading of my letters in the Academy. You indulge in a sneer at the character of those letters. With lips consequentially pouting and swelling with your important dictum, you exclaim, "O they were not read in the Academy, they were mere *verbiage*." It certainly is consistent in you to say they were not scientific, for you find it convenient not to take the least notice of the scientific details contained in those two papers, which were listened to with profound attention by the Academy. Of yourself, you are unequal to the task, though you assume the capacity of giving an opinion on the subject. You eschew the scientific parts of those papers, and carefully dwell on those which form a good ground-work for a *puff*. You are too clever by half, Mr. Briggs. With respect to the case of Captain W——, this young gentleman intrudes on me with his advice. I should forsooth have taken my information from Mr. Money or Mr. Green! Does he suppose that I have nothing else to occupy my time, than going about amongst my medical brethren, to ferret out the particulars of the cases treated by his patron? If he had been imbued with that candour for which I was willing to give him and his patron credit, he would have published in the article I am now occupied in answering, the letter which I addressed to Mr. Money on this subject. Such a course would have been manly and professional; but it would not have suited the left-handed policy of my antagonist. Here is the letter in question:—

"To —— Moore, Esq., 3, Hanover-street,

"My dear Sir,—A journey into the country yesterday prevented me from sending you the explanation which you required of me the day before. I could not under any circumstances refuse it, and less so when I consider the conciliatory and handsome manner in which you asked it from me. I therefore do not hesitate to avow, that I am opposed to the interpretation which M. Heurteloup supposes might be made of my statement of the case of Captain W. The only object I had in view, in my letter to the President of the French Academy, was to correct a mistatement made to that body, of the results of my practice in England. It was asserted that M. Heurteloup had succeeded with the curved percussion instrument, in two cases, in which I had previously failed. In order to prove the incorrectness of this statement, it became necessary to enumerate the cases in which M. Heurteloup had operated, after I had been previously consulted; this I did. I gave credit for one case, but I withheld it in the other. My defence before the Academy was called for, and I think I was bound in justice to myself to make it; and although this was the second trespass upon my reputation as an operator (I allude to the misrepresentation of the results of my practice, made in a letter of Mr. Alcock nearly three years ago), the tone of my letter was by no means acrimonious. It was even respectful towards the individual, although condemnatory of the instrument. I trust, whenever I am forced to figure in scientific controversy, that I shall always know how to maintain my opinions by fair argument, without degrading myself or my profession by vulgar or low abuse of persons.

"I do not think that we are in a condition to dogmatize upon the results of surgical operations, or that we are justified in saying, that because such a surgeon has operated, or such a mode of operation has been resorted to, or even that such an accident has arisen in the progress of an operation, that therefore the patient has lost his life; this being my opinion, how should I favour the interpretation, that because M. Heurteloup operated, therefore the patient died? I should be base, indeed, if I could act in this manner. This would be the logic of a furious partisan, and not of a sincere investigator of truth. This is my conscientious opinion. Now, I go farther, and state that if I were even convinced that the operation of M. Heurteloup, or any other man, was the cause of a patient's death, I should not say it. I will not, therefore, suffer such an interpretation to be put on my words, not only because it would be unjustly injurious to M. Heurteloup, but because it would be injurious to myself. Thanking you once more sincerely for your kindness on this occasion, believe me to be, dear Sir, yours, &c.

"W. B. Costello."

"7, Parliament-street, June 29, 1832.

With respect to the case of Mr. Stockbridge, he says, "he need make no comments on it; it speaks for itself." If he had said no more about it, I should have deemed my letter sufficiently long, and dismissed it also. As he has been so imprudent as to speak of it, I shall soon convince him that the less he said about it the better it would have been for the cause he attempts to advocate. "A straight instrument," says he, "could not be introduced into the bladder without violence, which, in this operation, above all others, should be avoided." Although Mr. Briggs is but a meagre authority on this matter, I agree with him, as to the necessity of avoiding all violence. *Sed cecidit in foveam quam fecit*; here he has dug the argument from under his feet. Let us compare what occurred to this patient under our respective treatments. M. Heurteloup introduced a straight instrument once, and attacked a stone, part of which was voided. During nearly six months following this sitting, many others were attempted, but they all failed. These attempts at introducing the instruments were productive of great distress, and were followed by swelling of the parts, by sanguineous infiltration, and leeches were frequently applied to combat the irritation. These attempts were at length abandoned, the impossibility of using a straight instrument was announced, founded on a presumed enlargement of the prostate, and a curved instrument was proposed.

I was at this time invited to see the patient, and although it is insinuated that undue means were had recourse to to bring this about, I declare that this insinuation is utterly unfounded. As I stated on a former occasion, I introduced the straight instrument twice, without violence, and without difficulty: upon the first occasion, to ascertain if it was possible; upon the second, to attack the stone. Upon neither of these occasions was there a drop of blood shed; upon neither was the injected water tinged; upon neither was pain given, upon neither did pain, swelling, or difficulty of making water, supervene. After the sitting, the fragments were voided without difficulty, and the patient resumed his chair as before. Now will Mr. Briggs answer, if any violence were used at all, against which of us will the evidence of it be the strongest? I did not say that M. Heurteloup used violence, I said he found it im-

possible to introduce the straight instrument. Mr. Briggs retorts and says, "if I succeeded where his patron failed, it could only have been by prevailing by the force of my hand and a metallic instrument over the soft and yielding organs; *for the Baron Heurteloup's acknowledged skill and talent could not have failed* in the usual application of this instrument." Harsh words are not necessary, neither shall I make use of them to wither the man, who could have written the preceding sentence. It comes to this pitch of arrogance,—that because M. Heurteloup failed to do this, no one else could!!!

Happily for me, I have in my possession the means of protecting myself against this malevolent insinuation. In the account of the autopsy, I stated that I preserved the parts; a minute's inspection of them will suffice to show that *the urethra was healthy and the prostate but very little enlarged.* I did not on the former occasion give the document sent to me from the medical men at full length, I shall now add a few lines which are appended to their account:—

"We the undersigned were present at the lithotritic operation performed by Mr. Costello on the late Mr. J. Stockbridge, and certify that it was performed with the greatest gentleness and skill, and with little apparent suffering to the patient. The time occupied was about three minutes.

"Signed, "Wm. C. Carver.
"Wm. Nunn, Surgeon.

"Royston, May 4, 1831."

This document was sent to me inclosed in the following letter:

"My dear Sir,—Inclosed is the document required. I should say to you, as a friend and well-wisher, publish the whole case, and I am quite sure it will tell to your advantage, and the sooner the better. You have nothing to suppress; everything was done that could be done agreeably to the present state of knowledge of your art. With best wishes for your success, believe me, my dear Sir, yours truly,

"Wm. Nunn."

With the account of the autopsy, and a few reflections which I shall now add, I think we shall have this case fairly before us.

It is an object to remove the stone from the bladder as quickly as circumstances will safely permit, not only because when it is in our power we should restore a sufferer to health with the least possible delay, but because, in the case we are considering, the evils arising from reflected irritation, are the great source of danger in this complaint. As it is admirably, and in accordance with physiological principles, laid down by Sir C. Bell, the urinary organs have throughout their different parts elective sympathies with each other; by elective sympathies we mean, that one portion of this apparatus will call another portion into a participation of its excitement or irritation, in preference to all other parts; thus the neck of the bladder, and the external orifice of the urethra, are in close sympathy, and whatever annoys the one is sure to involve the other in community of suffering. This is shown in common blenorrhagy. The orifice of the urethra is alone the seat of disease, yet micturition is frequent, and the neck of the bladder is the seat of different sensations of fulness, of tingling, or pain. Again, if the neck of the bladder be irritated, the orifice of the urethra becomes a large and faithful sharer of its distress. If the bladder be the seat of suffering, the sympathies of the kidneys are awakened; these organs are strained to overaction, excitement proceeds to sub-acute inflammation, enlargement, and alteration of their tissues, is the consequence, and thus the irritation of the bladder, which was the only disease primitively existing, by exciting the sympathies of another portion of the urinary apparatus, hurries it likewise into disease, and life is destroyed, not by the alteration which disease effects in the first affected organ, but by that which it induces in another organ of greater importance to the animal economy. An acute and sudden attack of nephritis is productive of the keenest suffering, while the alteration which is silently going on in the kidneys, the effect of sympathy with another portion of the urinary apparatus can scarce be suspected, much less diagnosticated. Pathological investigations fully authorise the foregoing observations. Now, from the autopsy of this case we find that death was caused by the disorganization of the kidneys. Was my operation, which was not painful, and which, locally, was followed by a soothing effect, the cause of this disorganization? No candid, no well-informed surgeon, will think so. How then does this case prove the utility of a curved instrument? The stone was broken by a straight one, and the cure would have been perfect if the kidneys had not been diseased!!!

Mr. Briggs says, in the statement made in the French Academy as to the cases in which his Patron was successful with the percussor, I was not the operator alluded to. I am fully aware that attempts have been made by other surgeons with the lithotrite, but is M. Heurteloup so uncandid as to conclude that because surgeons without adequate experience give lithotrity a trial, that their failures are to be ascribed to the inefficiency of the lithotrite? Dupuytren, Roux, and many others, have failed with

this instrument in cases where Civiale was afterwards successful. If he did not allude to me in the Academy, he ought to have said so distinctly.

After this comes a summing up under separate heads. 1st. He says it is impossible that I can have treated successfully with the instrument which he improperly calls *pierce-pierre* (for it crushes the stone as well as perforates it), the number of patients I name, because, adds he, it is natural to suppose, if I had effected these cures, I should have published them in detail.

This is tender ground for M. Heurteloup's advocate to tread upon.

A short time before M. Heurteloup came to this country, he stated to the Academy, without futher details, that he had operated in *twenty-three cases*. On the cure of his first case in London, he stated that it made his *forty-third* case. Of his French cases thirteen only are published, some, or most, of them under simple initials. Am I therefore to conclude he has not had forty-two cases in France? If I adopt the reasoning of his advocate, I ought to do so. I should not make this observation, if I were not forced to repel the doubt which it is so ignobly attempted to affix to my veracity. Persons most liable to deviate from truth, are often too apt to suspect the existence of their own failing in others. *Sed fas est ab hoste doceri.* He shall have my cases in full and honest detail.

2dly. According to him, I have never seen the curved instrument, &c. &c.

He will doubtless be surprised to learn that I have *seen it*, that I *possess* it, that I have *operated* with it, and what may grieve him still more to hear, that I have so modified, improved, and strengthened it, as to render it a safe and valuable instrument.

3dly. He says "It is difficult to conceive how Mr. Costello, in no way distinguished by scientific labours, and *comparatively* young as to experience, should, in so positive and unrestrained a manner, give his opinion on this operation," &c.

I fully concur with Mr. Briggs as to my being undistinguished in science, and of slender repute. However fortunate I may have been hitherto, or may be in future, in relieving the sufferings of my fellow-creatures, I shall always be ready to acknowledge the humbleness of my talents. It would be a curse to be endowed with but a moderate capacity, if vanity were allowed to exaggerate it into something stupendous. Still I must take leave, undistinguished though I be, to remark on the indecency of my being told so by a young man, who is still in the rudiments of the science. What he can mean by the words *comparatively young in experience*, I am not able to say. If the comparison be meant between his patron and myself, I must inform him that I have been longer at the profession than his patron, that I taught anatomy, ere yet his patron gave up his situation at the *Bureau de la Guerre* to study medicine, and that I have seen and done more of this operation than any other man living, Civiale alone excepted.

Lastly, he reflects on my delicacy for endeavouring to mar the rewards due to a professional brother; but he adds, when the facts are untrue (Are the facts untrue?) such conduct requires an appellation, which, out of respect, he refrains from applying. This is urged by the advocate of a man, who signalized himself by the acerbity of his *diatribes* against M. Civiale addressed to that same Academy, the effects of which were ten-fold more injurious to the author than the man against whom their shafts were aimed. Delicacy, indeed! When instruments long in existence are presented as inventions, accompanied by claims for reward; when such claims are substantiated at the expense of another's reputation, when theoretic perfections are urged forward, and practical defects concealed, it would be criminal in a man who humbly laboured in the cause of science and humanity, to withhold what he knew on the subject. Adopting the language of Mr. Briggs, I can say with reason, such conduct would indeed justify the appellation which he refrains from bestowing.

I have now done with this heap of letters. In replying to them, my greatest humiliation has been in finding myself compelled to descend to the tone in which they are written. I repeat it, a controversy which can only be carried on in terms of low personal invective, can have no bearing whatever on the objects I have in view. My profession of feelings on this point has been declared long since. The man who devotes himself to succouring his fellow-man, or to the improvement of his art, is respectable in my eyes. I feel towards him that interest, which community of pursuit and object inspires, and I wish him success as sincerely as I wish to be successful myself. From such a man I shall be ever ready to receive any useful suggestion, or real improvement. With such a man I can have no difference. But he who seeks fame to render it subservient to his cupidity, he who denies all merit in others to aggrandize his own, will ever be dispicable in my eyes, and whatever he asserts, I must scrutinize before I can believe it.

I have the honour to be, Sir, yours, &c.

WM. B. COSTELLO.

7, Parliament-street,
Monday, July 16, 1832.

THE LANCET.

London, Saturday, July 28th, 1832.

We take the earliest opportunity of calling the attention of the profession to the fact that the *Anatomy Bill* has passed the House of Lords, and ere this, probably, by receiving the ROYAL ASSENT, has become the law of the land. Regarding the provisions of this bill, we have had the misfortune to differ from, we believe, the whole of our contemporaries, a large majority of the members of the legislature, and the Council of the National Political Union. Time, the great Arbiter in such affairs, must now determine between us, as events will soon prove whether this measure was founded upon a correct calculation of facts, and the best means of satisfying the wants of the community, or whether it was framed in conformity with an imperfect knowledge of the existing law, and with ill-conceived opinions concerning the policy to be pursued. For ourselves, we may state unequivocally that our views remain wholly unchanged; we objected to the bill at first, we object to it still, because we can prove that it is an irrational measure, as founded upon the evidence given before the Parliamentary Anatomy Committee. Besides, we have ever contended that the security of the public against *burkers* could only arise out of the enaction of a measure which should go to the total prohibition of *buying*, or *selling*, dead human bodies. With such a conviction most firmly impressed upon our minds, we have, on a variety of occasions, urged our objections against the bill which has just received the sanction of the House of Lords. When, however, those objections were made on another occasion, we were told, with some degree of assurance, that the bill did *not* render the sale of dead bodies legal; but scarcely had this declaration annoyed the ears of those who are better informed, than the ATTORNEY GENERAL observed, in his place in parliament, that the bill *would* most certainly legalize the sale of dead human bodies. This point should be borne in mind.

In connexion, then, with this startling fact, let the members of the profession recollect what was the character of the evidence given against "resurrectionists,"—that is, against those persons who gain a livelihood by trading in the lifeless flesh and bones of their fellow-creatures. Were they not represented by Sir ASTLEY COOPER, Messrs. BRODIE, LAWRENCE, GREEN, and others, as the most abandoned of scoundrels? The worthy Baronet went even so far as to declare that, if they could *get anything by it*, they would not hesitate to make a subject of him at an hour's notice. How was this state of demoralization attempted to be explained? Instantly, by referring to the *trade* in which the resurrectionists were engaged; in truth, we should not be hazarding too broad an assertion by alleging, that the impression made upon the minds of the committee by the evidence given against the resurrection-men furnished a basis towards making them believe that any man who would *sell* a dead human body would not hesitate to murder a living one. Yet, now,—strange, unaccountable anomaly!—we have a bill, passed avowedly for the protection of the living, which renders it *legal* to carry on a traffic in the dead! The trade, then, is to receive a new impetus, and flourish more than ever! But we are informed that one member of the Government has said that the *conductors* of it will be changed, and that the public will derive security from the circumstance that under this Act of Parliament dead bodies will be sold chiefly by churchwardens, and overseers, and *medical practitioners*. Yes, one of the objects in passing this bill has been to transfer that species of commerce which has heretofore been carried on by resurrectionists into the hands

of members of the medical profession! Insulting, audacious proposition! Is there, we ask, a surgeon in England, Ireland, or Scotland, who is so lost to every honourable feeling, to every sense of decency and propriety, as to plunge himself so low in degradation by taking up a trade the carrying on of which has subjected other men to every term of reproach that could be applied to the most abandoned of human beings? The cutting edge of the gross insult offered in this supposition and insinuation is, however, completely turned aside by the circumstance that, during the progress of the Bill, not a single medical gentleman has petitioned in *favour* of that measure, whilst all petitions from practitioners,—as well from societies as from individuals,—earnestly prayed that such a traffic should be wholly prohibited by some pungent and dreaded penalty. Here, then, we have at once a convincing and cheering exposure of the infamous calumny uttered against medical men by a member of the legislature.

In a *reformed* House of Commons the most respectable and useful of all the professions will not be thus unblushingly insulted.

Satisfied, as we are, after the most mature consideration of the subject, that this bill, if it be permitted by the new parliament to remain unchanged or unrepealed, will produce in various parts of the kingdom the greatest possible degree of excitement and indignation, we have thought it right, at this first opportunity, to caution medical practitioners against being led away by their pure and disinterested love of science into certain paths of intrigue explored for them by the combined labours of boroughmongering legislators and certain trading anatomists. If proposals should at any time be made, in any of the hospitals, jails, penitentiaries, or workhouses, for disposing of the dead bodies therein, we implore medical practitioners to put the bridle upon their good intentions, and neither directly nor indirectly make themselves parties to any such transfers. Of course there will not be found persons sufficiently ignorant or daring to even hint at pecuniary recompense for engaging in such an agency. Our remarks extend to acts of interference of every description and under all circumstances, knowing as we do from past experience, that that medical attendant who may give a certificate of death preparatory to the removal of a body, will be deemed not less culpable than the parties actually making the sale, because it will be presumed that he does it with a foreknowledge that such sale is actually to take place, and that he voluntarily comes forward to knock down the only effectual obstacle to its perpetration. We would call the especial attention of our readers to this circumstance, because we have been informed, on the very best authority, that several of those members of the House of Commons who voted for Mr. Warburton's Bill, did so on the ground that exposures would not be of frequent occurrence, as the sales would be always managed in the workhouses, through the intervention of the medical attendants. That the Act cannot continue as now framed is certain, but as it is far from being worth the ruin of even a single medical gentleman's character, we have thus hastily and briefly pointed out to the profession the snares which are set to entrap them. Dead bodies may now be sold "according to law," and whoever lends a willing hand to the transmission of human "remains" to the dissecting room while such a state of the law exists, must expect to be branded as a person who has taken up the cast-off trade of the resurrectionists, and to be designated a "body-seller," if not a "body-snatcher." We shall, of course, procure a copy of the Act the moment that it is printed in its amended form, and examine at length the whole of its details

Observations on the General Principles and on the particular Nature and Treatment of various Species of Inflammation. By J. H. James, Surgeon to the Devon and Exeter Hospital, &c. Renshaw and Rush: London, 1832. 8vo. pp. 565. Price 15s.

Works in which the great principles of the medical and surgical art are laid down, explained, and fully developed, are scarce with us. Professional literature in England is mostly patch-work. We produce but very rarely indeed good radical works on the science of healing. In fact, we fall far short of our continental neighbours in every kind of production to which the press gives birth connected with the diffusion of anatomical and chirurgical knowledge. While the plates of Albinus, Scarpa, Mascagni, and Tiedemann, contribute to the extension of correct anatomical knowledge in other countries, we content ourselves with humble, and frequently worthless *copies*; or with the production of few and far between originals, which seldom rise above mediocrity. The plates of Mr. Shaw, illustrating the anatomy of the nervous system, are the only examples of national excellence on a large scale to which we can point. In like manner the standard works on anatomy generally, and in this country, are the composition of French and German authors. Until very lately, the names of Soemmering, Bichat, Meckel, and Cloquet, were our chief authorities upon points of general and descriptive anatomy. Fife, indeed, was warmly supported by the nationality of the Scotch, but to an extent which his real merits by no means deserved; and we might have still remained dependent on foreign assistance, had not Quain, Grainger, and Craigie, supplied, briefly, the deficiency in this branch of education. Our surgical writers also have been equally limited, if not in authority and value, at least in the magnitude of the subjects concerning which they have treated. The name of John Hunter certainly is known and respected by the most humble and distant cultivator of surgical knowledge; but we have perhaps no work which has taken an extensive range into our art, if we except the dictionary of Samuel Cooper, by far the best production of the kind in any language.

It will not be disputed, that a correct knowledge of the theory and treatment of inflammation is the corner-stone of surgery, embracing a vast extent and variety of topics, and requiring for its elucidation the utmost practical skill, joined with great literary acquirements, and patient industry. Yet how few of our countrymen have applied themselves to the investigation of this important subject! The celebrated work of John Hunter is chiefly confined to physiological and pathological details; the more comprehensive production of Mr. Thompson has, for some reason or other, been allowed to go out of print.* We are gratified, therefore, to find that a surgeon, so well qualified for the task as Mr. James, has at length undertaken the task, and furnished us with a comprehensive treatise on the nature of inflammation, and the general principles of treatment applicable to its numerous varieties.

The ground-work of the present volume is a thesis to which, in 1818, the Council of the College of Surgeons awarded the Jacksonian prize—a fact, however, which is of no moment in estimating the volume, for a more incompetent set of judges it would be difficult to select with a view to determine the value of anything but the current coin of the realm, or to ascertain how many pounds sterling can possibly be got for a worthless diploma. Mr. James has taken advantage of a dozen years of additional experience to correct and enlarge many of the views expressed in his thesis, and to modify his opinions by the increased light which the labours of Andral, and a host of others, have shed upon the subject of which he treats. From the nature of the volume, it is obviously impossible for us to offer any analysis of its contents. We rest content, therefore, with bestowing our commendation on the work, for the general accuracy of the opinions advocated in its pages, for the clear arrangement of the contents, and for the many valuable observations scattered throughout.

* A French translation of this excellent work may, we understand, be obtained for the small sum of nine shillings.

A Manual of Percussion and Auscultation, compiled from the French of MERIADEC LAENNEC. By JAMES BIRCH SHARPE, M. R. C. S. London: Highley. 1832. 12mo. pp. 118.

THIS excellent little volume consists of a translation of the summaries appended by Meriadec Laennec to the last edition of the great work of his illustrious relative, with some essential corrections, &c., by the translator. The whole has been revised by Dr. Elliotson, a circumstance which is sufficient to stamp the character of the work. The translation is faithful and perspicuous, and, altogether, Mr. Sharpe has succeeded in placing a cheap, portable, and valuable manual, within the reach of every member of the profession.

VISCERAL MALFORMATIONS IN SMITHERS, THE MURDERER.

THERE is at present preserved in the Museum of the London University a very complete specimen of that form of anomaly which arises from a change of position, or displacement of the thoracic and abdominal viscera. It occurred in the body of Smithers, the man who was executed a short time ago for having set fire to his house in Oxford Street, and thus caused the death of three of the inhabitants. The displacement consisted in a transposition of the principal viscera of the chest and abdomen from one side to the other. The heart appeared to occupy the centre of the thoracic cavity and its apex, instead of pointing to the left side, pointed directly downwards; the arch of the aorta formed a curve from the left to the right side of the bodies of the vertebræ, and the thoracic aorta descended for a short way on the right side of the spine; it then became placed on the centre of the bodies of the vertebræ, and ran down in the median line to the third lumbar vertebra, where it bifurcated in the usual manner. The right carotid and subclavian arteries arose by distinct trunks from the right side of the arch and the left carotid and subclavian, united at their origin into one trunk, formed an arteria innominata on the left side; the vena azygos ran up along the left side of the bodies of the vertebræ, but we could not learn whether the thoracic duct was displaced in a similar manner, as it was not preserved in the preparation. It is, however, probable, from the analogy of similar malformations, that the duct retained its relation to the vena azygos. The subdivision of the lungs into lobes was effected by the malposition of the heart; the right lung was divided into two lobes, and the left into three. In the abdominal cavity the transposition of organs was not less remarkable. The stomach and spleen were situate in the right hypochondriac region, and the liver filled up the whole of that space usually occupied by the stomach. The œsophagus penetrated the diaphragm to the right side of the aorta, while the opening for the vena cava was to the left. The cava itself ascended by the left side of the aorta to the anterior auricle, and sent off the right renal vein which curved over the aorta in a very conspicuous manner to the kidney. There was no anomaly of the genito-urinary apparatus. These displacements of the viscera are by no means uncommon. Sometimes we find only the contents of the chest displaced; sometimes only those of the abdomen; and occasionally, as in the present instance, the viscera of the chest and abdomen are transposed, at the same time from the left to the right side of the body, and vice versa. Indeed we may assert generally, that there is no thoracic or abdominal viscus, which may not be found in a different position from that which it usually occupies in the human body. In some rare cases the heart has been so far removed from its normal position as to be contained in the abdomen. An example of this kind is recorded by Deschamps.*

An old soldier, who was compelled to retire from the service by repeated attacks of nephritic pains, at length sunk under a very severe attack of the disease. The body was examined by Mons. Deschamps, and to the astonishment of the spectators, he found the heart situate in the left lumbar region; an opening in the diaphragm gave passage to the vessels of the lungs, head, and upper extremities.

Although it may seem strange to a non-medical reader, that life is consistent with the position of the heart in the belly, the possibility of its recurrence does not rest on this single fact. In the year 1788, Ramel examined with the greatest care the person of a young girl ten years of age, who had been subject from her infancy to palpitations and bleeding from the nose. The most accurate exploration of the chest failed to discover any pulsation within this cavity, but the contractions of the heart were visible both to the eye and touch, just below the diaphragm. The girl continued to enjoy pretty good health for a considerable time.*

* Observations sur un deplacement remarquable du cœur.—*Jour. Gen. de Med.* t. 25.

* Jour. de Med. Chir. et Phar. t. 49.

The literature of this country is rather scanty of information on the subject of monstrosities; we would refer our readers for a full and masterly account of every species of anomaly to the work lately published by J. G. Saint-Hilaire, entitled, *Histoire Generale et Particuliere des Anomalies*, &c. &c.

It is a work which every scientific man should peruse.

HYDRURET OF SULPHUR.

M. Thenard has lately published a very remarkable work on the hydruret of sulphur. This substance, obtained by Scheele, and examined by Berthollet and Berzelius, acquires great importance from the observations of Monsieur Thenard, who remarks, that perfect analogy exists between its properties, and those of oxygenated water.

The composition of hydruret of sulphur is very variable. M. Thenard found in it one atom of sulphuretted hydrogen, or its elements, and six or eight atoms of sulphur. To prepare, M. Thenard employs muriatic acid, diluted with twice its weight of water, and the hydruretted sulphuret of lime, obtained by boiling lime, with great excess of sulphur, in water. The acid being poured into a funnel, the aperture of which is corked, the sulphuret is afterwards very slowly added, taking care to stir the mixture continually. The hydruret of sulphur, which separates first, is more fluid than that which is afterwards obtained. Nothing is easier than to procure it in separate portions.

The hydruret of sulphur is liquid, but its consistence is very variable; it is yellow, sometimes with a tint of greenish brown. It whitens the tongue in the same manner as oxygenated water, and occasions a smarting sensation. It whitens and alters the skin rapidly. The colour of turnsall is immediately destroyed by it.

Its smell is peculiar and disagreeable. At 140° to 160° F. it begins to decompose into sulphur and sulphuretted hydrogen. When suffered to remain, it gives out from time to time bubbles of sulphuretted hydrogen, and sulphur only remains. Charcoal and several metals in powder disengage sulphuretted hydrogen. The same occurs with a great number of oxides, as the per oxide of manganese, silicia, the alkaline oxides; and what is very remarkable, potash and soda in solution produce the same effect.

The easily reducible oxides, as those of gold, silver, &c. occasion instantaneous ignition; the oxide is reduced, and water is formed.

All the sulphurets tend to disengage sulphuretted hydrogen. This effect is especially remarkable with the alkaline per sulphurets in solution; but then sulphur is deposited. Some organic matters tend to decompose hydruret of sulphur, but slowly. The action is rather stronger with animal substances; water and alcohol do not dissolve it; but it decomposes in them, though but tardily. Sulphuric ether dissolves it, and soon leaves crystals which appear to be sulphur.

Acids impart stability to the hydruret of sulphur; a little acidulated water is sufficient to produce this effect. In this case it not only does not decompose spontaneously, but the peroxide of manganese ceases to act upon it.

HYDRIODATE OF POTASH IN NEBULÆ OF THE CORNEA.

To the Editor of The Lancet.

Sir,—From the power which iodine possesses over the absorbents, I was induced to try a solution of hydriodate of potass in nebulæ of the cornea, and having used it in a variety of cases, I can confidently say that I have seen more benefit from it than from any of the usual applications.

I order half a drachm of the hydriodate of potass to be dissolved in one ounce of distilled water, to have the under lid depressed, and to apply a drop or two twice a day with a camel's-hair pencil; it occasions very little pain, and the person is able to walk about as usual in a minute or two after its being applied. Ulcers of the cornea also heal very quickly by using it.

In nebulæ of long standing, where the deposition has been very thick, after the outer edges have been absorbed, the centre often (from its density) remains obstinately fixed, and requires a stronger stimulus. I then apply, in the same way, either the liquor potassæ P. L., diluted with an equal quantity of distilled water, or the tincture of iodine, and by persevering in this plan, I have succeeded in many cases which before I should have considered *hopeless*.

Truly yours,
Edward Toole, M.R.C.S.L.

Bandan, June 13, 1832.

ADVANTAGES DERIVED BY THE SURGEONS AND ASSISTANT-SURGEONS OF THE ARMY, OVER THE SURGEONS AND ASSISTANT-SURGEONS OF THE NAVY.

To the Editor of THE LANCET.

SIR,—So much has been said and written upon the state of the medical profession in the navy, of late, that I have felt surprised mention has never been made respecting the superior condition of the medical members of the sister service.

It is unnecessary to say, that the requisite professional knowledge in the army and navy is identically the same; but the reward is widely different. The surgeon and assistant in the army, mess with their comparatives and superiors in rank in the army.

As gentlemen and officers they are entitled to attend the King's levee, a *favour* only lately granted to the naval surgeon and assistant. It is not long ago (before the exclusive order was revoked) that an assistant-surgeon in the army said to an old naval surgeon, "You may be my senior, but not my superior, as you can't attend the levee."

But the principal object of this letter is to point out the superior pay and half-pay of the army as compared with the navy, and in doing so I shall merely select the surgeon and assistant from each service.

Regimental Surgeon.

After 25 years actual service....	£1	2	0
After 20 but under 25 years	0	19	0
After 10 but under 20 years....	0	15	0
Under 10 years	0	13	0

Assistant-Surgeon of the Army.

After 25 years actual service....	0	10	0
After 20 but under 25 years....	0	10	0
After 10 but under 20 years....	0	10	0
Under 10 years	0	7	6

"Surgeons in the navy shall be paid all rates according to their length of service (including not more than three years as assistant-surgeon or hospital mate)"; so that from the expiration of their three years as assistant, until they are promoted, which is generally, on an average, six or eight years, the intermediate time is a hiatus in their service.

	Full pay per diem.		
Surgeons of less than 6 years' service....	0	10	0
Surgeons of more than 6 and less than 10 years' service	0	11	0
Surgeons of more than 10 and less than 20 years' service	0	14	0
Surgeons of more than 20 years' service..	0	18	0
Surgeons of hospital ships of whatever length of service	0	18	0
Assistant-surgeons of whatever length of service.....	0	6	6

Contrast this with the army. But the half-pay of the *sister* service presents a stronger proof that one sister is a greater favourite than the other.

Excerpt from the Warrant of July, 1830.

34. "The rate of half-pay for a medical officer of the army shall in every case be determined by the service which he may have rendered on full-pay, and the circumstances under which he may be placed on half-pay.

36. "If a medical officer shall be placed upon half-pay, by reduction of establishment, he shall be allowed the half-pay to which his service may entitle him, according to the following schedule, viz.

"*Rates of Half-pay after a Service on Full-pay.*

Regimental Surgeon.

	Per Diem.		
Thirty years	0	15	0
Twenty-five but under thirty years	0	13	0
Twenty but under twenty-five yrs	0	11	0
Ten but under twenty years....	0	8	6
Less than ten	0	6	0

Assistant-Surgeon.

Thirty years	0	7	6
Twenty-five but under thirty years	0	7	0
Twenty but under twenty-five yrs	0	6	0
Ten but under twenty	0	5	0
Less than ten	0	4	0

35. "If a medical officer shall be placed on half-pay from any other cause than reduction of establishment, he shall be allowed the half-pay to which his service may entitle him.

"*Rates of Half-pay after a Service on Full-pay.*

Regimental Surgeon.

	Per Diem.		
Thirty years	0	15	0
Twenty-five but under thirty years	0	11	6
Twenty but under twenty-five yrs.	0	8	0
Ten but under twenty	0	6	0
Less than ten	0	5	6

Assistant-Surgeon.

Thirty years	0	7	0
Twenty-five but under thirty years	0	6	0
Twenty but under twenty-five yrs.	0	5	0
Ten but under twenty years	0	4	0
Less than ten	0	3	0

"Every medical officer who may have served on full-pay for 25 years and upwards, shall have the right to retire upon half-pay at the rate assigned to his rank in the schedule subjoined to the 35th article, provided he shall have served three years in the rank from which he retires, but if he shall not have served three years in such rank, he shall receive only the rate attached to the rank from which he was last promoted.

"If a medical officer shall have served 30 years on full pay, including three in the rank from which he retires, or ten in the colonies, or five with an army in the field in any rank, or if his services be recommended under any special circumstance of merit, he may be allowed the rate of half-pay assigned to his rank in the schedule annexed to article 35.

"The rates of daily pay for the aforementioned ranks, are to be in future regulated by the length of time which the officers of each class shall have served on full pay, according to the annexed scale; provided always, that when any officer is hereafter promoted, he shall commence upon the minimum pay of his new rank, notwithstanding his length of service, agreeable to the said scale, may give him a claim to a higher rate of pay, as before he shall be allowed such higher rate of pay, he will be required to serve on each inferior rate of pay attached to his rank, for the following period, namely, one year if he had been in the medical department antecedently to the date of this warrant, and two years if he received his first medical commission subsequently to that date; but if the officer thus promoted had higher pay in his old rank than the minimum of his new rank, he shall commence upon that rate of pay which may be next above his former pay, and before he obtains any further increase, shall serve the period above prescribed, viz.—

"*Rates of Daily Pay subject to the above Provisions.*

Assistant-Surgeon.

After 25 years actual service ..	£0	10	0
After 20, but under 25 years....	0	10	0
After 10, but under 20 years....	0	10	0
Under 10 years	0	7	6

Regimental Surgeon.

After 25 years actual service ..	1	2	0
After 20, but under 25 years....	0	19	0
After 10, but under 20 years....	0	15	0
Under 10 years	0	13	0

Surgeons in the Navy.

	Half pay	per diem.	
After 6 years service (as surgeons)	0	6	0
Under that time	0	5	0

Assistant-Surgeons.

Three years service	0	3	0
Two years	0	2	0

After three years specified, his pay and service are stationary."

"Can such things be without our special wonder?"

In what essential are the medical officers of the navy deficient, that they should be treated thus? In what point as a body are they inferior in professional acquirements to their army brethren? And why are they to be rewarded inferiorly? What is the use of granting a new uniform to men who feel that it is only an additional expense, without conferring honour or rank, since they are still classed with warrant officers?

After a life spent in the service of their country, in all seasons and climates, are they not at least entitled to a compensation equivalent to their army confrères? Why is the period of service from the expiration of the first three years, until promotion is attained, not granted, to the naval assistant-surgeon? These are questions easier asked than answered. In the hope of seeing at a future period the services of naval medical officers placed on a similar footing to the army, I take my leave, and remain, Sir, yours truly,

CHIRURGUS.

July 18th, 1832.

EXTERNAL APPLICATION OF THE TARTAR EMETIC IN COLDS.

To the Editor of THE LANCET.

SIR,—A short time since I caught a violent cold on the lungs, accompanied by inflammation of the tonsils, rendering respiration and deglutition very difficult; without applying the usual remedies, I dissolved half a drachm of the antimonium tartarizatum in one ounce of water, and rubbed fifty large drops of the solution into my chest and throat at bed-time, which was about eleven o'clock. I slept as usual till four in the morning, when I awoke, and the application seemed to have produced little or no effect beyond creating a slight diaphoresis, which continued till seven, and then entirely left me. On the subsequent night I applied about eighty drops of the solution, containing, as near as I could calculate, eight grains of the tartar emetic; after a short time there were heat, redness, and itching, and nausea supervened, and about an hour afterwards a profuse perspiration came on. I now sank into a profound sleep, and about seven o'clock the next morning awoke and got up, the difficulty of breathing and impeded deglutition very greatly relieved, and my general health considerably improved. After repeating the dose on the following evening the complaint entirely left me, and my health was improved in a manner which astonished me.

One case affords but weak evidence of the efficacy of a medicine; but I think the decided benefit of the ant. tart. in the above case must be acknowledged. It cannot I think be doubted for a moment, that its ex-

ternal application (in certain cases) is highly preferable to its internal administration, particularly when used as an alterative. I intend, when I have an opportunity, to try its effects to a greater extent on myself. I remain, Sir, your obliged and humble servant,

R. Kilby.

South Lincolnshire, July 19, 1832.

LONDON HOSPITAL.

FEMORAL ANEURISM.

On Wednesday, July 4th, Mr. Andrews operated on W. R., ætat. 17, for femoral aneurism.

The lad stated, that about three weeks previous to his admission, while working at his business as a shipwright, he received a wound about the middle of the thigh from a chisel. The cicatrix is very evident; it is situated just about the middle of the thigh, over the femoral artery, where there is a pulsating tumour about the size of a small orange. On applying the stethoscope, the bruit de soufflet is distinctly heard. His general health is very good; appetite excellent; sleeps well; says that he feels no pain in the tumour, and that so far from experiencing any inconvenience from the aneurism, he should not know that any affection existed there, if he did not occasionally put his hand to the part.

On being placed on the table, and having compression made on the artery, some distance above the tumour, Mr. Andrews commenced by making an incision in the course of the vessel along the inner edge of the sartorious muscle. After exposing the femoral artery, there was considerable difficulty experienced in passing the aneurismal needle beneath the vessel, owing to the parts being obscured by a troublesome hemorrhage. After some time the tourniquet was applied, and ligatures were passed above and below the puncture; the artery was then divided between the ligatures. The edges of the wound were brought together, and retained by straps of adhesive plaster. On being removed to his bed, the limb was enveloped in tow.

On visiting him in the evening he was found quite comfortable and free from pain. The limb was of the natural heat, and the anterior and posterior tibial arteries could be plainly felt pulsating.

July 5. Passed a very good night, having slept very soundly for several hours; complains this morning of sickness of the stomach, with a hot skin and quick pulse; bowels not opened since yesterday. Was ordered the saline effervescing mixture, which has allayed the sickness, and he feels quite comfortable; the limb is rather hotter than its fellow. The pulsation in the arteries of the foot are less distinct than yesterday. Was ordered to take a dose of castor oil to-morrow morning, should the bowels not be freely opened without it.

6. Passed a very comfortable night, and is free from pain. His bowels not having been opened this morning, took the castor oil, which has operated very freely.

9. In every respect doing well. The wound was dressed to-day, and looks very healthy. The pulsations in the foot are less distinct, but can be felt; the limb is of the natural warmth.

Immediately after this patient was removed from the operating theatre, Mr. Hamilton proceeded to remove the left testicle of a man 22 years of age, affected with scrofulous disease of that organ. The operation was neatly performed; six ligatures were applied, the edges of the wound were brought together by four sutures, and straps of adhesive plaster. The patient has since gone on very well. The testicle was not examined.

On the 7th instant a man was admitted with compound comminuted fracture of the humerus, having fallen down a cellar while in a state of intoxication. The limb was placed in splints, and the patient was directed to keep quiet in bed. The following afternoon he became slightly incoherent. On the morning of the 9th, violent symptoms of delirium tremens came on; he was ordered 40 drops of laudanum, and a mixture composed of *Mist. camph. e ammon. carb.*, with *sp. æth. nitr.*, and also 16 ounces of wine. At one p.m. he died.

On Friday, 6th July, Mr. Scott operated for hydrocele of the spermatic chord, on a man ætat. 61. The disease has existed for about seven years. About fourteen ounces of fluid were evacuated.

On the same day that gentleman divided the sphincter ani of a man for a deep fistula in ano.

J. W., the lad whose leg was amputated by Mr. Luke on the 27th June last, continues daily improving. The stump looks very well; there is very little discharge. Seven ligatures have come away; his spirits are very good, and he rests well; bowels open without medicine.

The communication forwarded to The Lancet from the Central Board of Health, relative to the impunity with which fruit and vegetables may be eaten during the prevalence of cholera, reached us too late for publication in this number of the journal, but as the document is of public importance, it shall appear in The Ballot of to-morrow, and be farther disseminated in next week's Lancet.